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# ARTICLES

American Political Science Review Vol. 87, No. 2 June 1993

# MESSAGES RECEIVED: THE POLITICAL IMPACT OF MEDIA EXPOSURE

LARRY M. BARTELS *Princeton University*

*Analyses of the persuasive effects of media exposure outside the laboratory have generally produced negative results. I attribute such nonfindings in part to carelessness regarding the inferential consequences of measurement error and in part to limitations of research design. In an analysis of opinion change during the 1980 presidential campaign, adjusting for measurement error produces several strong media exposure effects, especially for network television news. Adjusting for measurement error also makes preexisting opinions look much more stable, suggesting that the new information absorbed via media exposure must be about three times as distinctive as has generally been supposed in order to account for observed patterns of opinion change.*

The state of research on media effects is one of the most notable embarrassments of modern social science. The pervasiveness of the mass media and their virtual monopoly over the presentation of many kinds of information must suggest to reasonable observers that what these media say and how they say it has enormous social and political consequences. Nevertheless, the scholarly literature has been much better at refuting, qualifying, and circumscribing the thesis of media impact than at supporting it. As Graber put it: "The findings that media effects were minimal were so pervasive in early research that after an initial flurry in the 1940s and 1950s, social science research into mass media effects fell to a low ebb. In study after study dealing with political socialization and learning, the mass media were hardly mentioned as an important factor" (1980, 10).

The field of electoral politics produced some of the most influential early findings of "minimal effects," especially in the classic Columbia studies of presidential campaigns in the 1940s (Berelson, Lazarsfeld, and McPhee 1954; Lazarsfeld, Berelson, and Gaudet 1948). Subsequent research, when it was conducted at all, generally produced similar results. Even the dramatic rise of television did little to alter the scholarly consensus regarding the role of the mass media. The thesis of a widely cited presidential campaign study conducted a generation after the Columbia studies (Patterson and McClure 1976) is nicely conveyed by its subtitle, *The Myth of Television Power in National Elections.*

Social scientists have occasionally acknowledged that the persuasive effects of the mass media may be more fugitive than minimal. For example, Arterton noted with apparent equanimity the disjuncture between what social scientists have demonstrated and what campaigners believe about media effects: "Political scientists studying the impact of listening to or reading reported news have been unable to document significant effects upon the attitudes, cognitions, or behavior of citizens. The effect may be there, but we have not been able to demonstrate it. In any case, political science findings notwithstanding, those who manage presidential campaigns operate on the conviction that what the media say about them will affect their candidates' votes on election day" (1978, 4).

The scholarly effort to document significant media effects has been bedeviled by a variety of methodological difficulties. In aggregate-level time-series analyses, it is usually impossible to distinguish the effects of the media themselves from the effects of the events they report. In individual-level cross-sectional studies, differences in opinions between those exposed to the media and those who remain unexposed may simply reflect preexisting differences between the two groups in political attitudes or characteristics. Self-reports of media exposure may be biased in ways that produce artificial correlations with political opinions, especially when the media exposure questions being used refer to specific candidates or issues. For all of these reasons, findings of significant impacts and minimal effects alike have seldom been wholly convincing.

Experimental research elegantly avoids many of these inferential pitfalls. Thus, it should not be surprising that the most convincing demonstrations of media exposure effects to date have come from laboratory settings (Iyengar 1991; Iyengar and Kinder 1987). Nevertheless, experimental methods have their own considerable limitations, primarily with respect to external validity. For this reason, if no other, it behooves us to explain—and to reassess—the pervasive pattern of negative findings and nonfindings in the nonexperimental literature on media effects.

The present work attributes this pervasive pattern of negative findings and nonfindings in part to limitations of research design and in part to carelessness regarding measurement. I do not intend to suggest that all of the findings of minimal effects in the existing scholarly literature are due simply to methodological limitations. Indeed, my own analysis of a varied collection of political opinions and perceptions in one eight-month presidential campaign season provides ample evidence that media exposure only occasionally produces strong, unidirectional opinion changes. Part of my aim in analyzing changes in a broad range of specific campaign opinions and perceptions is to present a clearer sense of just how common such strong, unidirectional media exposure effects may be in a presidential campaign setting. But in addition, I hope to present a clearer sense of both why such effects are not more common than they are and why they are more common than previous analyses have generally been able to demonstrate. By reconceptualizing what sorts of media effects we might expect to find and how we might expect to find them given the nature of the available data, I attempt to cast both new and old evidence in a very different light.

## A MODEL OF MEDIA EFFECTS

According to Graber, "People who are exposed to the mass media already possess a fund of knowledge and attitudes which they bring to bear on new information. Since we do not know precisely what this information is, nor the rules by which it is combined with incoming information, we cannot pinpoint the exact contribution which mass media make to the individual's cognitions, feelings, and actions" (1980, 11). I propose a model of opinion formation that can help to "pinpoint the exact contribution which mass media make to the individual's cognitions, feelings, and actions" by overcoming precisely the difficulties Graber identifies.

The "fund of knowledge and attitudes" respondents bring to bear as they are exposed to the media is addressed in the model by incorporating preexisting opinions as explanatory variables. Obviously, this approach requires repeated interviews with a panel of survey respondents.

Even with access to panel data, "we do not know precisely what this information is" because our measurements of preexisting knowledge and attitudes are usually quite imperfect. It should be well known (e.g., from Achen 1983) that measurement error in any explanatory variable will generally bias parameter estimates for every explanatory variable in a multiple regression model. Since analyses of media impact typically include explanatory variables subject to serious measurement error (including media exposure itself and, in panel studies, lagged values of the dependent variable), the biases introduced by treating all of these observed variables as error-free indicators of the underlying opinions and behaviors are likely to be both substantial in magnitude and unpredictable in direction. The analytic implications of measurement error are addressed here by distinguishing, both in the formulation of the model and in the subsequent data analysis, between the underlying variables of theoretical interest and observable indicators of those variables.

Finally, "the rules by which [preexisting information] is combined with incoming information" are modeled by the assumption that respondents use new information from the mass media and elsewhere to update their political opinions rationally in accordance with Bayes' Rule. Bayes' Rule may or may not be a realistic behavioral model; but it is certainly a useful accounting device—in particular because it provides a systematic way to characterize both the relative weight of old and new information in people's current opinions and the nature and sources of the new information they have absorbed between any two opinion readings.

My model represents an individual i's opinion about some political stimulus j at time t as a Normal probability distribution with mean $\theta_{ijt}$ and variance $1/\pi_{ijt}$. (It may be helpful to think of $\theta_{ijt}$ as representing the "location" of the opinion and $\pi_{ijt}$ as representing the certainty or "precision" of the opinion.) Given Bayesian updating, the relationship between this opinion at time t and the corresponding opinion at any previous time s is

$$\theta_{ijt} = \theta_{ijs}\pi_{ijs}/(\pi_{ijs} + \omega_{ijt}) + \mu_{ijt}\omega_{ijt}/(\pi_{ijs} + \omega_{ijt})$$

$$\pi_{ijt} = \pi_{ijs} + \omega_{ijt},$$

where $\mu_{ijt}$ and $\omega_{ijt}$ represent the location and precision, respectively, of a Normal probability distribution representing new information (a "message") received between time s and time t. The precision of the opinion at time t, $\pi_{ijt}$, is equal to the sum of the prior precision ($\pi_{ijs}$) and the message precision ($\omega_{ijt}$); the location of the opinion at time t, $\theta_{ijt}$, is a weighted average of the prior location ($\theta_{ijs}$) and the message location ($\mu_{ijt}$), each weighted by its relative precision.

This model is obviously too general as it stands, since nothing in it is directly observable and everything varies both across individuals and over time. We might make some progress by introducing observable measures of subjective information corresponding to the unobserved variables $\pi_{ijs}$ and $\pi_{ijt}$; but the resulting nonlinear model is difficult, in practice, to estimate. In view of this difficulty, a tempting alternative approach is to treat the ratio $\pi_{ijs}/\pi_{ijt}$ as a constant parameter $\lambda_{jt}$ for all i. This assumption implies that individuals vary in how much they know (or rather, in how much they think they know) at any given time, but that the amount of new (subjective) information acquired by any individual in a given time interval is proportional to the amount of (subjective) information that individual already possesses at the beginning of the interval.[1] Adopting this simplification gives

$$\theta_{ijt} = \theta_{ijs}\,\lambda_{jt} + \mu_{ijt}(1 - \lambda_{jt}), \qquad (1)$$

which represents each individual's opinion at time t ($\theta_{ijt}$) as the same weighted average of prior opinions ($\theta_{ijs}$) and new information ($\mu_{ijt}$).

I assume that the relationships between the unobserved variables $\theta_{ijt}$ and $\mu_{ijt}$ and the observed data are of the forms

$$\theta_{ijt} = Y_{ijt} - \delta_{ijt}$$

and

$$\mu_{ijt} = (\mathbf{X_{it}} - \zeta_{it})\boldsymbol{\alpha}_{\mathbf{jt}} + \varepsilon_{ijt}, \qquad (2)$$

where $Y_{ijt}$ is an opinion reading (including measurement error); $\mathbf{X}_{it}$ is a vector of observations (including measurement error) of exogenous characteristics related to the message that individual i absorbs about stimulus j at time t; $\delta_{ijt}$ and $\boldsymbol{\zeta}_{it}$ are random variables representing the measurement error in the observed data $Y_{ijt}$ and $\mathbf{X}_{it}$, respectively; $\varepsilon_{ijt}$ is a random variable representing additional components of the message $\mu_{ijt}$ absorbed by individual i; and $\boldsymbol{\alpha}_{jt}$ is a vector of parameters to be estimated. Thus, the mean of the distribution representing opinion at time t is assumed to be measured directly but with error by the observed variable $Y_{ijt}$, whereas the mean of the distribution representing the new information received between time s and time t is assumed to be unobservable but linearly related to a vector of exogenous characteristics, which are in turn measured directly but with error by the observed variables $\mathbf{X}_{it}$.

Substituting these relationships into equation 1 and rearranging gives

$$\begin{aligned} Y_{ijt} &= (Y_{ijs} - \delta_{ijs})\lambda_{jt} + (\mathbf{X}_{it} - \boldsymbol{\zeta}_{it})\boldsymbol{\alpha}_{jt}(1 - \lambda_{jt}) \\ &\quad + \varepsilon_{ijt}(1 - \lambda_{jt}) + \delta_{ijt} \\ &= (Y_{ijs} - \delta_{ijs})\lambda_{jt} + (\mathbf{X}_{it} - \boldsymbol{\zeta}_{it})\boldsymbol{\beta}_{jt} + u_{ijt}. \qquad (3) \end{aligned}$$

This model takes the relatively simple form of an errors-in-variables regression model with dependent variable $Y_{ijt}$, explanatory variables $Y_{ijs}$ (with associated parameter $\lambda_{jt}$) and $\mathbf{X}_{it}$ (with associated parameter vector $\boldsymbol{\beta}_{jt} = \boldsymbol{\alpha}_{jt}(1 - \lambda_{jt})$), and disturbance term $u_{ijt} = \varepsilon_{ijt}(1 - \lambda_{jt}) + \delta_{ijt}$.[2]

## DATA

The data employed here to estimate the parameters of the model are from the 1980 American National Election Study (NES) panel.[3] The data consist of a variety of opinion readings at three time points for a national cross-section of 758 survey respondents (the survivors from a first-wave sample of 1,008). The first wave of interviews was conducted in late January and February (before the first primary voting in New Hampshire), the second wave in June (between the end of the primary season and the national nominating conventions), and the third wave in September (during the first month of the general election campaign).

My analysis utilizes two distinct measures of media exposure. The only relevant item included in the three waves of the 1980 NES survey employed here focuses specifically upon exposure to television network news: "How often do you watch the national network news on early evening TV—every evening, 3 or 4 times a week, once or twice a week, or less often?" The mean levels of network news exposure derived from translating responses to this question onto a 0 (minimum exposure) to 1 (maximum exposure) scale declined slightly over the course of the campaign season, from .71 in February to .66 in June to .64 in September.[4]

In the first two waves of the 1980 NES survey, respondents were also asked, "Do you read a *daily* newspaper regularly?" This question was omitted in the third wave of the survey but included again in a fourth (postelection) wave (albeit without the adverb), making it possible to exploit the availability of three-wave panel data for newspaper exposure, as well as television news exposure. Moreover, newspaper reading appears to be a sufficiently stable behavior to warrant using June exposure as a proxy for September exposure in the analysis that follows.[5]

In order to allow for the importance of partisan learning and reinforcement effects (Berelson, Lazarsfeld, and McPhee 1954; Conover and Feldman 1989; Finkel 1990), all of the analyses reported here incorporate party identification as an exogenous influence on the nature of the message $\mu_{ijt}$ received during each time period.[6] Partisan predispositions are measured quite reliably by the traditional party identification item in the NES survey[7] (recoded here to range from −1 for "strong Democrats" to +1 for "strong Republicans") and are exceedingly stable over the course of the campaign season.[8]

Most previous analyses of media exposure effects have been vulnerable to the argument that the apparent effects of media exposure actually reflect the impact of politically relevant social characteristics that happen to be correlated with media exposure. For example, since older people, the well-educated, and blacks are all disproportionately likely to watch television or read newspapers, any systematic opinion changes among these groups might easily be mistaken for effects of media exposure.[9] The argument is less compelling when party identification is already included as an explanatory variable, since the most likely source of systematic opinion change among particular groups in a political campaign is partisan "activation" (Berelson, Lazarsfeld, and McPhee 1954). Nevertheless, to further guard against the possibility of estimating spurious media effects, all of the analyses reported here include age, education, and race as additional exogenous control variables.[10]

The effects of media exposure were separately estimated for each half of the campaign year (February–June and June–September) for each of 37 distinct perceptions and opinions regarding the presidential candidates, their character traits, their issue positions, the respondents' own issue preferences, and (in the case of incumbent Jimmy Carter) various aspects of job performance. To facilitate comparison, all of the original responses were recoded to range from 0 to 100, with 0 denoting the most negative possible opinion and 100 the most positive possible opinion.[11]

The key to dealing with the biases created by measurement errors in explanatory variables is to obtain estimates of the magnitudes of those measurement errors. Repeated measurement of the same opinion or behavior at three or more time points provides leverage for distinguishing between change in underlying "true" opinions and random measurement error. Here, the magnitudes of measurement errors are estimated using a variant of the Wiley and Wiley (1970) model.[12] The main assumptions underlying the model are that the measurement process produces constant error variance in each wave of the panel and that measurement errors for the same respondent in different waves of the panel are uncorrelated.[13]

Responses to the media exposure questions appear to contain a moderate amount of measurement error. Applying the modified Wiley–Wiley measurement model to the three available waves of panel data, the standard errors of measurement (the square roots of the estimated measurement error variances) on the 0–1 scale are .16 for television exposure and .23 for the dichotomous measure of newspaper exposure. The corresponding "reliabilities" of the exposure measurements (the ratios of "true" variance to total variance) are .75 for television exposure and .78 for newspaper exposure, which suggests that about a quarter of the observed variance in these variables represents random noise.

Descriptive statistics for all variables in each wave of the panel, as well as measurement error estimates calculated from the modified Wiley–Wiley model, are presented in Appendix A. The standard errors of measurement vary from 10 or 12 points on the 100-point scale (for overall "thermometer" ratings) to 20 or 25 points (for Carter job approval). The corresponding measurement reliabilities also vary widely, from a high of .84 (for Carter thermometer ratings) to a low of .29 (for placements of Carter on the "government spending vs. services" issues scale); average reliabilities are about .61 for Carter job approval, .58 for issue preferences, .57 for candidate traits, and .46 for candidate issue placements. These reliabilities are, in most cases, considerably lower than for media exposure and party identification, and suggest that with the notable exception of the thermometer ratings, about 40% of the observed variance in the political perceptions represents random noise.

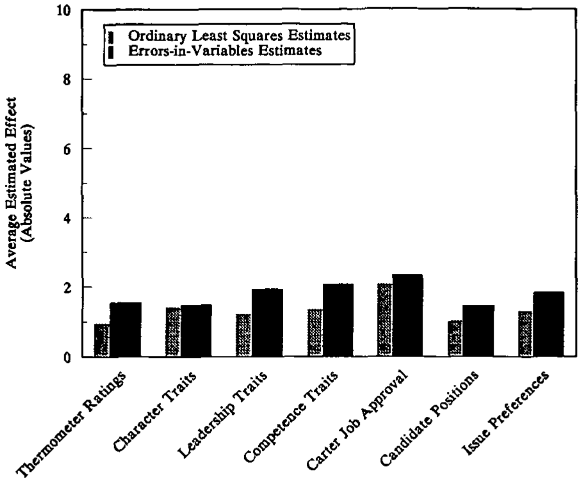
## ESTIMATED EFFECTS OF MEDIA EXPOSURE

Errors-in-variables parameter estimates for the model of opinion change I have proposed earlier are reported in Appendix B for both campaign periods.[14] Figures 1 through 4 present the average estimated effects of television news exposure, newspaper exposure, party identification, and prior opinions from Appendix B for seven subsets of dependent variables: thermometer ratings, character traits, leadership traits, competence traits, Carter job approval ratings, candidate issue positions, and respondents' own issue preferences.[15]

The average magnitudes of the television news exposure effects in Figure 1 range from about 3 points on the 100-point scale in the case of thermometer ratings and issue placements to about 6 points in the case of competence traits and Carter approval ratings.[16] The newspaper exposure effects in Figure 2 follow a similar pattern but are, on average, about half as large, ranging from 1.5 points for thermometer ratings and issue positions to less than 2.5 points for Carter approval ratings.[17]

FIGURE 1

**Summary of Estimated Effects of Television News Exposure**



Effects of the magnitude portrayed in Figure 1 are especially impressive when we bear in mind that even "full exposure" to network television news represents, at most, a few hours of relevant coverage of each candidate spread over the entire campaign year. Robinson and Sheehan's content analysis of the news media during the 1980 campaign suggests that a faithful viewer of *The CBS Evening News* from January through October was exposed to a total of about 15½ hours of presidential campaign coverage, of which about 10 hours were devoted to the "horse

FIGURE 2

**Summary of Estimated Effects of Newspaper Exposure**

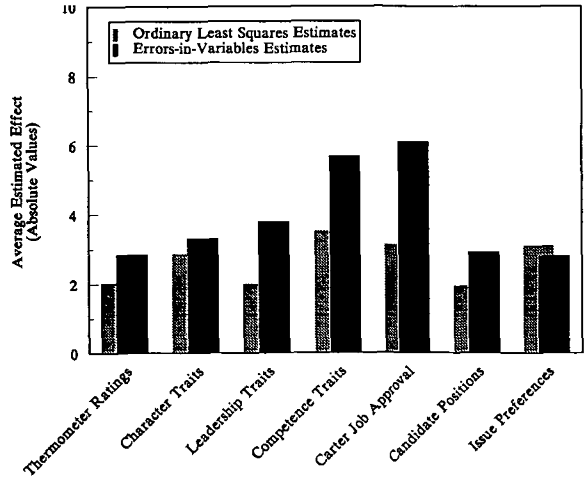
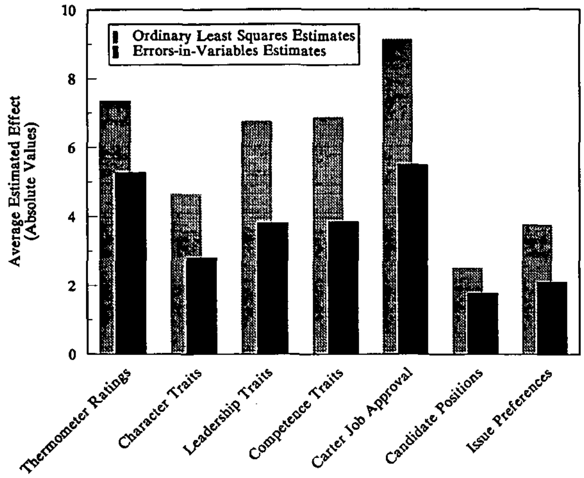


FIGURE 3

**Summary of Estimated Effects of Party Identification**

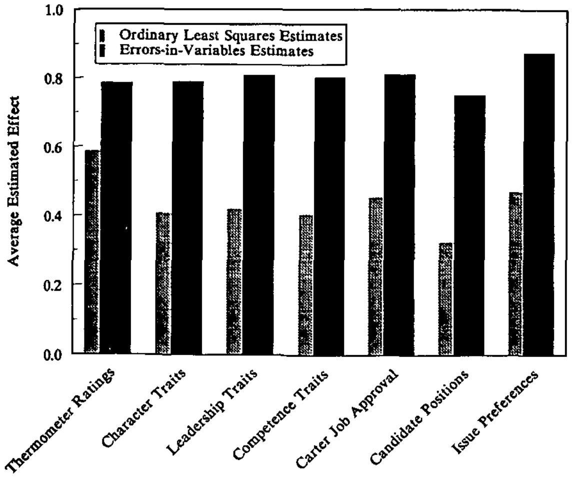


race" and about 5½ to "candidate information" and "policy issues" (1983, 149). Moreover, a significant fraction of that coverage occurred in the last month of the campaign, after the last interviews analyzed here. Even with some allowance for the effects of other sources of information that are likely to be correlated with exposure to television network news (most notably, debates and other public affairs broadcasts and advertising), the tendency of television viewers to make distinctive inferences about the candidates' personal traits, performance, and issue positions on the basis of relatively modest amounts of coverage is striking.

For purposes of comparison, Figures 1–4 also summarize the ordinary least squares estimates corresponding to the errors-in-variables estimates in Appendix B.[18] It is clear from the figures that ordinary least squares estimates generally understate the effects of television news and newspaper exposure, in some cases by as much as 50%, while overstating the impact of party identification on opinion change and, most dramatically, understating the persistence of prior opinions. The magnitude and diversity of these divergences between ordinary least squares and errors-in-variables estimates highlight the inferential hazards of naive empirical analysis when explanatory variables are measured with significant error (Achen 1983).

FIGURE 4

**Summary of Estimated Effects of Prior Opinions**



The statistical fit of the model estimated in Appendix B (including prior opinions, party identification, demographic controls, and television news and newspaper exposure) is excellent.[19] But despite the overall goodness of fit of the model, none of the individual media exposure parameter estimates in Appendix B is very precise. More than half are smaller than their standard errors, since the standard errors themselves average almost 4 points on the 100-point scale for the television exposure effects and almost 2.5 points for the newspaper exposure effects.

However, it would be rash to infer from the prevalence of "insignificant" parameter estimates that there really are no underlying media exposure effects to be found. This is clear from further analysis of the candidate trait variables, where the availability of several measures of essentially similar traits can be exploited to refine the estimates of media exposure effects. For example, treating the "moral," "dishonest," and "power-hungry" traits explicitly as aspects of a more general *character* dimension makes it possible to estimate the effects of media exposure on perceptions of character more precisely than with the specific responses taken separately. The same is true for a *leadership* dimension made up of the "weak," "inspiring," and "strong leader" traits and for a *competence* dimension consisting of the "knowledgeable," "solve economic problems," and "develop good relations" traits.

Table 1 presents the results of covariance structure analyses for these three trait dimensions for each candidate. For each trait dimension, estimated effects of television news exposure, newspaper exposure, party identification, and prior opinion comparable to those reported in Appendix B are presented both for June (top) and September (bottom). Additional details of the covariance structure analysis, including the estimated relationships between each general trait dimension and its specific observable indicators, are reported in Appendix C.

Grouping the character traits into broader evaluative dimensions reduces the average standard error of the media exposure parameter estimates by almost a third. The result is to highlight the general pattern of media exposure effects already evident in Appendix B. Thus, for some trait dimensions, the additional precision of the parameter estimates makes it even clearer in Table 1 than in Appendix B that media exposure did, indeed, produce minimal opinion change. In other cases, the results in Table 1 make it even clearer that the observed effects of media expo-

TABLE 1

Covariance Structure Parameter Estimates for Carter and Reagan Trait Dimensions

| TRAIT DIMENSIONS | TV NEWS EXPOSURE | NEWS EXPOSURE | PARTY ID | PRIOR OPINION | S.E. OF REGR. | $X^2_{(134)}$ |
|---|---|---|---|---|---|---|
| | | | June | | | |
| **Carter** | | | | | | |
| Character | −1.1 | −.7 | −1.6 | .820 | | |
| | (2.8) | (1.7) | (1.2) | (.076) | 10.5 | 297.0 |
| Leadership | −2.6 | −3.2 | −4.1 | .787 | | |
| | (2.7) | (1.7) | (1.2) | (.050) | 9.1 | 272.7 |
| Competence | −4.8 | −3.5 | −2.3 | .955 | | |
| | (2.7) | (1.6) | (1.2) | (.069) | 3.7 | 333.2 |
| **Reagan** | | | | | | |
| Character | −.2 | .5 | 2.0 | .774 | | |
| | (2.5) | (1.5) | (1.0) | (.064) | 8.1 | 333.3 |
| Leadership | .4 | .7 | 5.1 | .709 | | |
| | (2.5) | (1.5) | (1.0) | (.057) | 9.8 | 295.9 |
| Competence | 4.1 | 1.8 | 5.1 | .764 | | |
| | (2.5) | (1.5) | (1.1) | (.078) | 8.0 | 282.3 |
| | | | September | | | |
| **Carter** | | | | | | |
| Character | 3.8 | 1.6 | −1.2 | .802 | | |
| | (2.8) | (1.5) | (1.1) | (.059) | 8.2 | 297.0 |
| Leadership | .4 | 1.8 | −1.9 | .883 | | |
| | (2.8) | (1.6) | (1.2) | (.051) | 7.0 | 272.7 |
| Competence | 4.2 | 1.0 | −2.3 | .995 | | |
| | (3.0) | (1.7) | (1.2) | (.069) | 4.8 | 333.2 |
| **Reagan** | | | | | | |
| Character | .9 | 3.6 | 5.6 | .872 | | |
| | (2.7) | (1.5) | (1.1) | (.065) | 7.4 | 333.3 |
| Leadership | −7.8 | .1 | 3.3 | .859 | | |
| | (2.8) | (1.6) | (1.2) | (.066) | 10.4 | 295.9 |
| Competence | −9.9 | −.4 | 5.4 | .817 | | |
| | (2.9) | (1.7) | (1.4) | (.078) | 10.1 | 282.3 |

Note: Parameter estimates are analogous to those presented in Appendix B. Components of trait dimensions are specified in Appendix C. Intercepts and parameter estimates for age, education, and race are not shown. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.

sure are much too large to be attributed to mere sampling variability. Even in the case of newspaper exposure, where the effects are generally much smaller than for television news exposure, the three largest of the 12 t-statistics in Table 1 are 2.4, 2.2, and 1.9, much larger than could be expected on the basis of chance if all of the true effects were zero. It seems clear from this more refined analysis that the results in Appendix B should likewise be read as evidence of some very strong media exposure effects *and* some minimal effects, with a liberal admixture of pure noise due to the inherent imprecision of the individual parameter estimates.

## MESSAGES RECEIVED

The results summarized in Figures 1 and 2 demonstrate that attention to measurement error is sufficient by itself to produce some upward revision in the apparent impact of media exposure. But the implications of the analysis presented here for our understanding of media effects are considerably greater than the comparisons in Figures 1 and 2 alone suggest. A more significant—but also more subtle—revision in our understanding of the role of the media is necessitated by the comparison in Figure 4, which indicates that allowing for the effects of measurement error produces a picture of much more stable political opinions over the course of a campaign season.

The comparison summarized graphically in Figure 4 is represented somewhat differently in the first two columns of Table 2, which compares the errors-in-variables and ordinary least squares estimates of the fraction of total information apparently gained during the campaign (i.e., between the first wave of NES interviews in February and the final preelection wave in September) for each of the 37 opinions and issue

**TABLE 2**

**Campaign Information and Distinctive Campaign Messages, February–September 1980**

| | CAMPAIGN INFORMATION | | TV NEWS EXPOSURE | | NEWS. EXPOSURE | | PARTY ID | |
|---|---|---|---|---|---|---|---|---|
| DEPENDENT VARIABLES | *EV* | *OLS* | *EV* | *OLS* | *EV* | *OLS* | *EV* | *OLS* |
| **Thermometer ratings** | | | | | | | | |
| Carter | .372 | .606 | 1.0 | 4.8 | −5.5 | −1.0 | −25.1 | −20.3 |
| Reagan | .408 | .704 | 14.4 | 5.0 | 7.2 | 2.0 | 23.3 | 15.4 |
| **Carter job approval** | | | | | | | | |
| Overall | .380 | .727 | −4.7 | 6.3 | 1.3 | −3.0 | −31.7 | −23.2 |
| Iran | .490 | .809 | −5.8 | .0 | −12.1 | −4.0 | −27.5 | −18.5 |
| Inflation | .192 | .769 | −16.2 | .5 | −29.1 | −5.1 | −44.4 | −20.7 |
| Unemployment | .455 | .826 | −11.1 | −1.2 | −8.7 | −2.9 | −27.2 | −16.6 |
| Energy | .261 | .844 | −2.2 | 1.6 | −23.1 | −5.9 | −28.0 | −15.7 |
| **Issue positions** | | | | | | | | |
| Lib./con. ideology | .073 | .718 | 4.5 | −.1 | 10.3 | 4.0 | 46.8 | 10.0 |
| Spending vs. services | .306 | .826 | −2.3 | −4.1 | 2.7 | −1.2 | 7.8 | 9.8 |
| Defense spending | .248 | .741 | 22.8 | 11.9 | −10.1 | −1.7 | 12.3 | 5.3 |
| Relations with Russia | .318 | .819 | −14.2 | .4 | 5.7 | 2.3 | 1.2 | 2.7 |
| | | | | **Carter** | | | | |
| **Traits** | | | | | | | | |
| Moral | .218 | .794 | 3.5 | 4.6 | 4.9 | 4.3 | −7.5 | −5.7 |
| Dishonest | .536 | .895 | −19.5 | −8.2 | 1.1 | −.7 | 4.7 | 4.5 |
| Power-hungry | .394 | .802 | .5 | 2.6 | −4.1 | −2.8 | 9.3 | 8.3 |
| Weak | .388 | .838 | 4.8 | −1.6 | .8 | −1.9 | 12.7 | 11.1 |
| Inspiring | .352 | .826 | .8 | 5.1 | −4.5 | −.9 | −17.2 | −12.1 |
| Strong leader | .381 | .747 | −3.2 | 2.4 | −2.8 | .5 | −18.7 | −14.6 |
| Knowledgeable | .449 | .855 | 23.8 | 13.6 | −2.4 | .4 | −8.5 | −8.7 |
| Solve economic problems | .282 | .820 | −14.5 | .9 | −12.2 | −1.4 | −10.4 | −12.6 |
| Develop good relations | .184 | .828 | −15.7 | −.3 | −7.8 | −2.2 | −28.5 | −13.4 |
| **Positions** | | | | | | | | |
| Lib./cons. ideology | .206 | .850 | −.7 | .6 | 4.8 | .2 | −23.5 | −7.9 |
| Spending vs. services | .363 | .937 | −16.5 | −5.0 | −2.9 | −1.7 | .7 | −.2 |
| Defense spending | .463 | .916 | −1.1 | 1.0 | 2.6 | 2.8 | −4.8 | −5.5 |
| Relations with Russia | .804 | .975 | −5.6 | −.2 | 2.3 | 2.2 | −5.6 | −5.1 |
| | | | | **Reagan** | | | | |
| **Traits** | | | | | | | | |
| Moral | .349 | .832 | −1.8 | 3.8 | 15.6 | 3.9 | 17.6 | 9.3 |
| Dishonest | .522 | .869 | −11.0 | −9.1 | −8.8 | −4.5 | −11.5 | −7.8 |
| Power-hungry | .228 | .796 | 3.8 | −1.9 | −4.9 | −.5 | −46.2 | −17.0 |
| Weak | .333 | .942 | 25.3 | 3.6 | −15.2 | −4.8 | −17.5 | −8.1 |
| Inspiring | .484 | .745 | −19.5 | −5.2 | 1.0 | −1.1 | 19.7 | 13.7 |
| Strong leader | .191 | .798 | −35.7 | −4.2 | .4 | −1.3 | 40.2 | 14.1 |
| Knowledgeable | .460 | .826 | −12.1 | −1.0 | 6.3 | 3.0 | 13.9 | 9.0 |
| Solve economic problems | .460 | .838 | −19.8 | −6.3 | 5.6 | .3 | 21.9 | 13.8 |
| Develop good relations | .327 | .846 | −16.8 | −4.4 | −1.5 | −.8 | 34.4 | 16.5 |
| **Positions** | | | | | | | | |
| Lib./cons. idelogy | .195 | .762 | 15.1 | 2.8 | 5.0 | 1.4 | 17.0 | 5.3 |
| Spending vs. services | .458 | .892 | −8.8 | −2.2 | −3.5 | −1.8 | 4.3 | 2.1 |
| Defense spending | .558 | .871 | 8.6 | 3.9 | −.7 | −.9 | 3.1 | 2.2 |
| Relations with Russia | .438 | .899 | −12.2 | −.2 | −3.4 | −.9 | −3.9 | −1.8 |

*Note:* "Campaign information" is the proportion of total September information received between February and September, estimated by $(1 - \hat{\lambda}_2 \hat{\lambda}_3)$, where $\hat{\lambda}_2$ and $\hat{\lambda}_3$ are the estimated effects of February opinions in June and of June opinions in September, respectively. "Distinctive messages" are differences in the content of messages received between February and September attributable to television news exposure, newspaper exposure, and party identification, estimated by $(\hat{\beta}_2 \hat{\lambda}_3 + \hat{\beta}_3)/(1 - \hat{\lambda}_2 \hat{\lambda}_3)$, where $\hat{\lambda}_2$ and $\hat{\lambda}_3$ are the estimated effects of February opinions in June and of June opinions in September, respectively, and $\hat{\beta}_2$ and $\hat{\beta}_3$ are the relevant estimated exposure effects in June and September, respectively. Errors-in-variables (EV) entries are based upon the parameter estimates in Appendix B. Ordinary least squares (OLS) entries are based upon analogous ordinary least squares regression estimates.

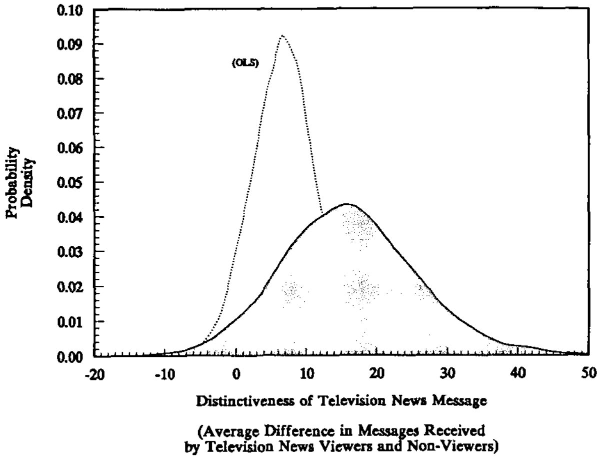
perceptions included in the analysis.[20] Since total information is, by definition, the sum of preexisting information and campaign information, the increased weight of preexisting information, once we take account of measurement error, necessarily produces a corresponding decrease in the relative weight of campaign information. The ordinary least squares estimates suggest that on average, more than 80% of the total information that respondents had at the time of the third NES interview in September had been gained since the first interview in February. The implication of these estimates is that campaign impressions dominate electoral politics, at least at the presidential level. By contrast, the errors-in-variables estimates suggest that on average, only a little more than one-third of the total information that respondents had in September had been gained since February. By these latter estimates, most of what people believed about both Carter and Reagan in the midst of the general election campaign was already fixed months earlier, before the public phase of the campaign had even begun.

The difference between these two sets of estimates is of profound significance for any understanding of the electoral process. The tendency of measurement error to depress the apparent stability of political opinions has been widely recognized (Achen 1975; Feldman 1989). However, the implication of this tendency for analyses of opinion change has not generally been recognized. In terms of the Bayesian model of opinion change proposed earlier, new information must compete with a much greater mass of prior information than has generally been supposed and thus must itself be much more distinctive than has generally been supposed in order to produce the opinion changes that we actually observe. Thus (perhaps counterintuitively), evidence that preexisting opinions are very stable also suggests, albeit indirectly, that the political information required to produce systematic changes in those opinions must be very distinctive.

One advantage of the proposed Bayesian model is that it allows us to characterize precisely the new information, or "messages," required to account for observed opinion change during the course of a campaign. Moreover, the formulation in equation 2 makes it possible to assess how much these messages vary with media exposure, party identification, and other characteristics. When the (hypothetical) messages required to account for observed opinion changes vary systematically with media exposure, the implication is that media users were absorbing *distinctive* messages unavailable to those who remained unexposed.

FIGURE 5

Estimated Distinctiveness of Television News Message for Reagan Thermometer Ratings, February–June 1980



This implication of the analysis is elaborated in the remaining columns of Table 2, which present estimates of the distinctiveness of the messages received during the 1980 campaign *from* television news and newspapers and *by* partisan identifiers, respectively.[21] For example, each entry in column 3 is an estimate of the difference between the messages received by a regular viewer of the network news and a nonviewer between February and September. In the notation of the model, these are estimates of the $\alpha_{jt}$ parameters for the effect of television news exposure on the location of the received message $\mu_{ijt}$ in equation 2.[22]

The individual estimates vary a great deal and most are in any case quite imprecise. But the average difference in the messages received by regular television news viewers and nonviewers over this eight-month period is clearly on the order of 10 points on the 100-point scale. The corresponding average difference for regular newspaper readers and nonreaders is about half as large, while the average difference attributable to partisan predispositions is on the order of 15 or 20 points on the 100-point scale.[23]

The standard errors of these "distinctive message" estimates are impossible to calculate directly, since the estimates themselves are based on ratios of correlated parameter estimates from Appendix B. However, a good sense of the sampling variability of the estimates can be built up empirically by repeated sampling from the original correlated distributions. Figure 5 displays the estimated sampling distribution of message distinctiveness constructed in this way for a single, roughly typical case—the impact of television news exposure on changes in Reagan thermometer ratings between the February and June waves of the 1980 NES panel.[24]

For purposes of comparison, Figure 5 also displays the estimated sampling distribution of message distinctiveness based upon the ordinary least squares estimates of the same television news exposure effect. The specific example nicely illustrates the general tendency for ordinary least squares to underestimate the distinctiveness of the messages received by people regularly exposed to the mass media during the 1980 campaign. It was clear from Figures 1–4 that ordinary least squares significantly underestimates both the impact of media exposure and the stability of preexisting opinions. The estimated message distinctiveness compounds these two underestimates, producing results that fall short by 57% in this case and by 67% on average. Thus, the messages conveyed by the media in the 1980 presidential campaign were about three times as distinctive as they appear from simple regression analysis.

## CONCLUSION

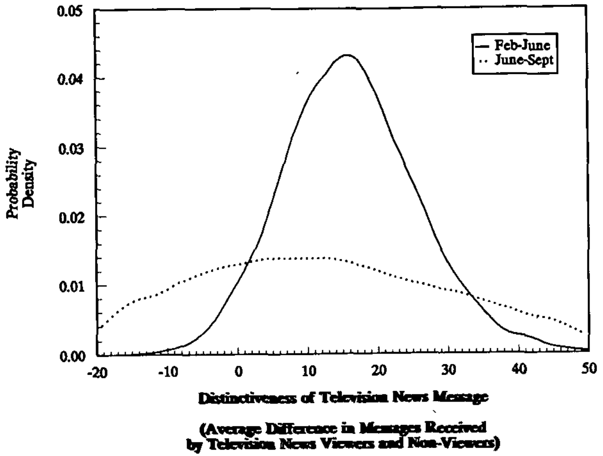
Attention to the effects of measurement error significantly increases the apparent impact of media exposure on opinion change in a presidential campaign setting. Nevertheless, to the extent that analysts focus upon observable opinion change over relatively short periods of time, the apparent effects of media exposure will often be modest in magnitude even when adjusted for the effects of measurement error—not because the media cannot be persuasive but because opinions at the beginning of a typical presidential campaign are already strongly held and because media messages during the course of the campaign are, in any case, only occasionally sharply inconsistent with those preexisting opinions. By the logic of rational (Bayesian) opinion change, only discrepant messages can produce observable change—and then only in direct proportion to the subjective uncertainty of preexisting opinions.

Media exposure is not directly implicated in the formation of these preexisting opinions themselves. But then, there is no way that it could be, given the nature of the available data.[25] A panel spanning eight months is far from a snapshot, but it is too truncated a moving picture to capture the full effect of media exposure on opinions and perceptions that were already well developed at the beginning of this eight-month period. In the absence of any direct access to the process of opinion formation prior to the campaign season, the most natural supposition is that it, too, was heavily dependent upon media exposure. However, the apparent stability of political opinions in a presidential campaign setting suggests that more direct and convincing demonstrations of significant opinion changes due to media exposure will require data collections spanning considerably longer time periods.

The logic of Bayesian opinion change pursued here also suggests that media exposure is most likely to be consequential (in the sense of producing large observable opinion changes) when prior opinions are weak, most notably for "new" candidates or issues. For this reason, simply as a matter of efficiency, analysts of media effects would do well to focus upon "new" or "uncrystallized" opinions, even if they are atypical or intrinsically less significant than opinions that are better established and more firmly held.

FIGURE 6

**Estimated Distinctiveness of Television News Message for Reagan Ratings, February–June and June–September 1980**



This point is illustrated in Figure 6, which compares the estimated sampling distribution of television news "message distinctiveness" for Reagan thermometer ratings between February and June 1980 (reproduced from Figure 5) with the corresponding sampling distribution of message distinctiveness between June and September. The central tendencies of the two distributions suggest that the distinctive messages received from network television news were not wildly different for the two parts of the campaign; the medians for the two sampling distributions are 15.7 and 9.9, respectively. However, the sampling distribution for September is so diffuse that it provides little useful information about the real distinctiveness of the message received from television news during the latter part of the campaign. (More than 30% of the mass of the sampling distribution actually falls outside the range of values represented in Figure 6.) The main cause of this diffuseness is the significantly greater stability of prior opinions about Reagan in September than in June. As a result, the estimated impact of television exposure, which had a t-ratio of 1.8 in June, had a t-ratio of .4 by September—not because there was no impact to be measured, but because the stability of preexisting opinion by September reduced that impact to a level below the threshold of precision of the available data. Attempting to study media impact in settings with very stable prior opinions is a social scientist's equivalent of attempting to count galaxies through the wrong end of a telescope.

Finally, the analysis presented here highlights the political significance of distinctive, consistent media messages presented over relatively long periods of time. Many of the media exposure effects evident in the 1980 presidential campaign were not, ultimately, consequential in the final election outcome, either because positive effects in one period canceled out negative effects in another period (as with the effects of television news exposure on perceptions of Carter's competence and job performance) or because similar effects of roughly equal magnitude for both candidates canceled each other out. Consistent, distinctive media messages favoring one side or the other in a political controversy are, by contrast, likely to produce sizable opinion changes over time (Zaller 1992). Thus, studies of when and why such consistent, distinctive media messages get produced should be among the highest priorities for research on the political impact of the mass media.

The distinctive campaign messages attributed to media exposure in Table 2 are doubly imprecise, since they compound the imprecision of the estimated media exposure and prior opinion effects in Appendix B. The imprecision of the individual estimates is mitigated, for present purposes, by the consistency of results across a quite varied collection of campaign opinions and perceptions. But for more detailed analyses of specific cases, more precise estimation of the messages received by different survey respondents will be of crucial importance.

Better data will also be required to analyze the distinctive effects of various sorts of direct and indirect media exposure. Obviously, not all of the effects portrayed here are specifically attributable to network news and newspapers, since respondents most exposed to these specific media were probably also most exposed to debates, candidate advertisements, and other sources of campaign information. More elaborate media exposure data will be required to sort out the effects of these various information sources more fully.[26] On the other hand, comparisons based upon direct media exposure probably understate to a considerable extent the total impact of the media, since they make no allowance for indirect exposure of the sort posited by Barr's Law.[27] Better data may eventually make it possible to trace these indirect effects of the media upon people who are not directly exposed to their distinctive messages.

Much remains to be done. Nevertheless, the view that media exposure has "minimal effects" in political campaigns appears, on the basis of the analysis presented here, to be due in significant part to inattention to the implications of measurement error, combined with the tendency of previous research designs to focus upon significant short-term opinion change in circumstances where such change is likely to be quite modest. More careful analysis, together with an alternative focus upon the distinctiveness of the messages received by people exposed to the media, casts the pervasive political impact of the modern mass media in a clearer light.

## APPENDIX A

**TABLE A-1**

**Descriptive Statistics and Measurement Error Estimates**

| VARIABLE | MEANS S.D. FEB. | JUNE | SEPT. | STD. ERROR OF MEASUREMENT | AVERAGE COEFFICIENT OF RELIABILITY |
|---|---|---|---|---|---|
| Television news exposure | .708<br>.317 | .662<br>.316 | .644<br>.315 | .158 | .75 |
| Newspaper exposure | .621<br>.485 | .600<br>.490 | .623<br>.476 | .227 | .78 |
| Party identification | −.124<br>.657 | −.099<br>.676 | −.116<br>.678 | .231 | .88 |
| Carter overall job approval | 55.3<br>36.6 | 36.9<br>35.6 | 39.2<br>36.7 | 19.7 | .71 |
| Carter approval | | | | | |
| Iran | 57.2<br>42.1 | 33.6<br>39.8 | 35.5<br>39.0 | 24.8 | .62 |
| Inflation | 34.1<br>34.3 | 26.2<br>32.9 | 32.9<br>34.0 | 21.3 | .60 |
| Unemployment | 47.7<br>33.5 | 30.1<br>32.4 | 35.0<br>33.6 | 21.1 | .59 |
| Energy | 34.6<br>36.7 | 27.1<br>34.0 | 40.2<br>35.2 | 24.4 | .52 |
| Issue Preferences | | | | | |
| Lib./con. ideology | 54.4<br>17.6 | 53.7<br>18.5 | 55.2<br>18.5 | 11.0 | .63 |
| Spending/services | 42.6<br>28.4 | 44.1<br>27.8 | 45.9<br>27.1 | 18.2 | .57 |
| Defense spending | 69.1<br>23.0 | 65.6<br>23.3 | 68.6<br>22.7 | 14.5 | .60 |
| Relations with Russia | 53.2<br>32.6 | 54.6<br>30.0 | 54.4<br>28.4 | 20.7 | .53 |

**TABLE A-1 continued**

**Descriptive statistics and Measurement Error Estimates**

| VARIABLE | MEANS S.D. FEB. | JUNE | SEPT. | STD. ERROR OF MEASUREMENT | AVERAGE COEFFICIENT OF RELIABILITY |
|---|---|---|---|---|---|
| Carter | | | | | |
| Thermometer rating | 63.5<br>24.3 | 53.3<br>26.4 | 56.2<br>26.3 | 10.4 | .84 |
| Moral | 74.1<br>23.5 | 70.8<br>24.4 | 70.8<br>24.5 | 15.9 | .57 |
| Dishonest | 16.4<br>23.7 | 21.3<br>24.3 | 24.8<br>23.4 | 17.0 | .49 |
| Power-hungry | 31.9<br>29.4 | 39.6<br>30.4 | 44.8<br>29.8 | 19.0 | .60 |
| Weak | 40.7<br>30.1 | 49.7<br>29.7 | 44.9<br>26.9 | 19.7 | .53 |
| Inspiring | 45.9<br>29.0 | 38.7<br>26.4 | 41.1<br>26.1 | 17.9 | .56 |
| Provide strong leadership | 47.2<br>27.1 | 37.0<br>27.0 | 40.3<br>27.4 | 14.9 | .70 |
| Knowledgeable | 65.3<br>25.2 | 59.8<br>26.4 | 61.1<br>23.8 | 17.4 | .52 |
| Solve economic problems | 40.0<br>25.0 | 32.0<br>23.2 | 34.3<br>24.2 | 15.7 | .58 |
| Develop good relations | 55.3<br>27.1 | 44.2<br>27.3 | 48.7<br>27.6 | 18.8 | .53 |
| Lib./cons. ideology | 53.1<br>17.9 | 49.2<br>20.0 | 47.6<br>18.1 | 13.5 | .47 |
| Govt. spending/services | 38.1<br>20.6 | 41.5<br>20.4 | 40.1<br>19.1 | 16.8 | .29 |
| Defense spending | 59.2<br>21.4 | 51.0<br>20.7 | 50.8<br>19.9 | 15.9 | .41 |
| Relations with Russia | 44.9<br>28.8 | 40.9<br>24.8 | 42.2<br>22.6 | 19.6 | .39 |
| Reagan | | | | | |
| Thermometer rating | 52.5<br>23.6 | 58.4<br>23.3 | 56.2<br>25.0 | 11.7 | .76 |
| Moral | 62.6<br>22.2 | 63.7<br>21.8 | 61.5<br>23.1 | 14.6 | .57 |
| Dishonest | 27.5<br>23.7 | 26.5<br>22.7 | 25.6<br>24.0 | 15.2 | .58 |
| Power-hungry | 48.9<br>28.2 | 49.1<br>28.0 | 53.2<br>30.4 | 18.8 | .57 |
| Weak | 31.6<br>23.2 | 28.8<br>21.7 | 28.6<br>24.7 | 18.0 | .39 |
| Inspiring | 45.9<br>25.7 | 48.1<br>25.8 | 44.8<br>27.6 | 13.5 | .74 |
| Provide strong leadership | 52.5<br>23.9 | 55.5<br>24.5 | 51.7<br>27.3 | 16.3 | .58 |
| Knowledgeable | 61.0<br>21.8 | 59.3<br>22.9 | 58.5<br>26.0 | 14.3 | .63 |
| Solve economic problems | 46.3<br>22.5 | 49.4<br>22.5 | 44.2<br>24.2 | 14.4 | .61 |
| Develop good relations | 49.0<br>23.3 | 51.2<br>22.9 | 46.3<br>26.0 | 15.7 | .57 |
| Lib./cons. ideology | 60.4<br>21.3 | 63.3<br>21.8 | 62.6<br>20.8 | 13.6 | .59 |
| Govt. spending/services | 53.7<br>19.8 | 54.7<br>19.8 | 55.5<br>21.3 | 14.4 | .50 |
| Defense spending | 60.0<br>20.5 | 61.9<br>19.5 | 69.6<br>21.4 | 13.0 | .59 |
| Relations with Russia | 55.4<br>22.6 | 54.2<br>20.8 | 57.1<br>21.6 | 15.7 | .47 |

*Note*: All variables except network news exposure (0–1), newspaper exposure (0–1), and party identification (−1–1) are recoded to vary between 0 and 100. Number of observations = 758.

## APPENDIX B

**TABLE B-1**

**Errors-in-Variables Parameter Estimates, June 1980**

| DEPENDENT VARIABLES | TV NEWS EXPOS. | NEWSP. EXPOS. | PARTY ID | FEB. OPIN. | S.E. OF REGR. | $X^2_{(30)}$ |
|---|---|---|---|---|---|---|
| | | | Thermometers | | | |
| Carter | −2.6<br>(3.1) | .5<br>(1.9) | −5.9<br>(1.4) | .810<br>(.055) | 13.3 | 61.1 |
| Reagan | 5.2<br>(3.0) | 3.0<br>(1.8) | 7.8<br>(1.3) | .667<br>(.051) | 11.7 | 30.8 |
| | | | Carter Approval | | | |
| Overall | −9.5<br>(4.8) | 1.1<br>(3.0) | −6.1<br>(2.1) | .683<br>(.057) | 17.9 | 56.6 |
| Iran | −4.9<br>(5.9) | −2.6<br>(3.6) | −7.1<br>(2.3) | .557<br>(.053) | 23.2 | 37.2 |
| Inflation | −3.4<br>(4.8) | −0.8<br>(2.9) | −2.2<br>(2.2) | .818<br>(.071) | 11.0 | 49.5 |
| Unemployment | −14.1<br>(5.0) | −0.6<br>(3.0) | −2.8<br>(2.0) | .594<br>(.069) | 18.0 | 48.9 |
| Energy | 6.0<br>(5.3) | −3.7<br>(3.2) | −4.4<br>(2.3) | .739<br>(.073) | 11.2 | 55.8 |
| | | | Issue Positions | | | |
| Lib./cons. ideology | −.6<br>(2.6) | −1.7<br>(1.6) | 4.1<br>(1.1) | .927<br>(.070) | 5.5 | 49.3 |
| Govt. spending vs. services | −5.6<br>(4.1) | −.1<br>(2.6) | .6<br>(2.0) | .781<br>(.098) | 11.6 | 44.6 |
| Defense spending | 1.4<br>(3.4) | −1.0<br>(2.1) | 2.8<br>(1.3) | .893<br>(.083) | 8.0 | 39.2 |
| Relations with Russia | .4<br>(4.6) | −2.7<br>(2.8) | 3.4<br>(1.8) | .738<br>(.065) | 10.0 | 50.8 |
| | | | Carter Traits | | | |
| Moral | −3.3<br>(3.8) | .7<br>(2.3) | −2.7<br>(1.6) | .892<br>(.121) | 8.9 | 51.2 |
| Dishonest | −5.8<br>(3.9) | .3<br>(2.4) | 2.8<br>(1.6) | .678<br>(.141) | 12.3 | 43.8 |
| Power-hungry | 2.9<br>(4.6) | 1.7<br>(2.8) | .1<br>(2.0) | .753<br>(.095) | 15.8 | 40.9 |
| Weak | 5.0<br>(4.3) | 2.5<br>(2.7) | 6.5<br>(1.9) | .735<br>(.074) | 10.9 | 43.1 |
| Inspiring | −3.6<br>(3.7) | −.9<br>(2.3) | −5.7<br>(1.6) | .690<br>(.063) | 7.5 | 50.8 |
| Provide strong leadership | .6<br>(3.7) | −4.3<br>(2.3) | −4.5<br>(1.6) | .686<br>(.063) | 13.5 | 48.3 |
| Knowledgeable | 3.4<br>(4.1) | .3<br>(2.5) | −.8<br>(2.1) | .877<br>(.139) | 10.4 | 38.2 |
| Solve economic problems | −5.1<br>(3.4) | −4.9<br>(2.1) | −4.4<br>(1.4) | .725<br>(.074) | 7.1 | 47.5 |
| Develop good relations | −7.0<br>(4.2) | −3.7<br>(2.5) | −2.1<br>(1.9) | .844<br>(.104) | 5.6 | 50.9 |

**TABLE B-1 continued**

**Errors-in-Variables Parameter Estimates, June 1980**

| DEPENDENT VARIABLES | TV NEWS EXPOS. | NEWSP. EXPOS. | PARTY ID | FEB. OPIN. | S.E. OF REGR. | $X^2_{(30)}$ |
|---|---|---|---|---|---|---|
| | Carter Issue Positions | | | | | |
| Lib./cons. ideology | −4.8 | .7 | −2.9 | 1.000 | 6.2 | 42.1 |
| | (3.2) | (2.0) | (1.3) | (.008) | | |
| Govt. spending vs. services | −2.8 | .1 | −.9 | .835 | 5.6 | 33.2 |
| | (3.5) | (2.2) | (1.4) | (.165) | | |
| Defense spending | −2.1 | 2.4 | −1.3 | .662 | 8.5 | 33.5 |
| | (3.3) | (2.1) | (1.5) | (.115) | | |
| Relations with Russia | −5.5 | −1.4 | −2.2 | .267 | 12.8 | 48.6 |
| | (4.0) | (2.4) | (1.6) | (.057) | | |
| | Reagan Traits | | | | | |
| Moral | 1.6 | .4 | 1.4 | .768 | 8.3 | 30.8 |
| | (3.3) | (2.0) | (1.4) | (.092) | | |
| Dishonest | −2.5 | −.6 | −1.9 | .577 | 11.8 | 39.4 |
| | (3.5) | (2.1) | (1.4) | (.082) | | |
| Power-hungry | 3.4 | −.4 | −3.7 | .812 | 10.0 | 37.9 |
| | (4.2) | (2.6) | (1.8) | (.089) | | |
| Weak | −.4 | .3 | −2.9 | .667 | 4.8 | 31.4 |
| | (3.6) | (2.2) | (1.5) | (.127) | | |
| Inspiring | −1.3 | −.7 | 6.6 | .668 | 14.3 | 39.8 |
| | (3.5) | (2.2) | (1.4) | (.063) | | |
| Provide strong leadership | 1.5 | 1.4 | 5.3 | .809 | 9.4 | 33.7 |
| | (3.7) | (2.3) | (1.6) | (.098) | | |
| Knowledgeable | 3.8 | 1.4 | 2.5 | .661 | 13.5 | 33.9 |
| | (3.5) | (2.2) | (1.5) | (.110) | | |
| Solve economic problems | .6 | 5.4 | 6.9 | .654 | 10.8 | 31.6 |
| | (3.2) | (2.0) | (1.5) | (.083) | | |
| Develop good relations | 6.0 | −.5 | 5.8 | .780 | 7.2 | 36.1 |
| | (3.4) | (2.1) | (1.5) | (.099) | | |
| | Reagan Issue Positions | | | | | |
| Lib./cons. ideology | −1.4 | 1.6 | 2.1 | .908 | 6.2 | 30.1 |
| | (3.1) | (1.9) | (1.2) | (.093) | | |
| Govt. spending vs. services | −3.9 | .1 | −.4 | .763 | 5.6 | 33.0 |
| | (3.1) | (1.9) | (1.2) | (.148) | | |
| Defense spending | 3.4 | −1.9 | 2.0 | .630 | 8.5 | 36.0 |
| | (2.9) | (1.7) | (1.1) | (.086) | | |
| Relations with Russia | .2 | −4.2 | 1.6 | .562 | 12.8 | 39.4 |
| | (3.3) | (2.0) | (1.3) | (.076) | | |

*Note*: Intercepts and parameter estimates for age, education, and race not shown. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.

**TABLE B-2**

### Errors-in-Variables Parameter Estimates, September 1980

| DEPENDENT VARIABLES | TV NEWS EXPOS. | NEWSP. EXPOS. | PARTY ID | FEB. OPIN. | S.E. OF REGR. | $X^2_{(30)}$ |
|---|---|---|---|---|---|---|
| | | | Thermometers | | | |
| Carter | 2.4<br>(2.9) | −2.4<br>(1.7) | −4.8<br>(1.3) | .776<br>(.045) | 10.9 | 61.1 |
| Reagan | 1.2<br>(3.3) | .3<br>(1.9) | 2.6<br>(1.6) | .887<br>(.067) | 11.3 | 30.8 |
| | | | Carter Approval | | | |
| Overall | 6.8<br>(4.8) | −.5<br>(2.8) | −6.5<br>(2.2) | .908<br>(.068) | 9.6 | 56.6 |
| Iran | 1.6<br>(5.7) | −3.5<br>(3.3) | −6.9<br>(2.2) | .915<br>(.049) | <1 | 37.2 |
| Inflation | .2<br>(5.1) | −4.8<br>(2.9) | −6.4<br>(2.1) | .987<br>(.057) | <1 | 49.5 |
| Unemployment | 7.9<br>(5.2) | −3.4<br>(2.9) | −9.8<br>(2.0) | .917<br>(.099) | 8.5 | 48.9 |
| Energy | −6.6<br>(5.8) | −2.3<br>(3.4) | −2.9<br>(2.2) | 1.000<br>(.018) | <1 | 55.8 |
| | | | Issue Positions | | | |
| Lib./cons. ideology | .9<br>(2.7) | 2.4<br>(1.6) | −.7<br>(1.0) | 1.000<br>(.026) | <1 | 49.3 |
| Govt. spending vs. services | 4.2<br>(4.3) | .9<br>(2.5) | 1.9<br>(1.9) | .888<br>(.092) | 6.6 | 44.6 |
| Defense spending | 4.5<br>(3.5) | −1.6<br>(2.0) | .7<br>(1.3) | .842<br>(.064) | 7.0 | 39.2 |
| Relations with Russia | −4.9<br>(4.8) | 4.3<br>(2.8) | −2.8<br>(1.8) | .923<br>(.061) | <1 | 50.8 |
| | | | Carter Traits | | | |
| Moral | 3.7<br>(4.0) | .4<br>(2.3) | .7<br>(1.6) | .877<br>(.090) | 9.4 | 51.2 |
| Dishonest | −6.5<br>(4.0) | .4<br>(2.2) | .6<br>(1.6) | .685<br>(.120) | 9.9 | 43.8 |
| Power-hungry | −2.1<br>(4.6) | −3.0<br>(2.7) | 3.6<br>(1.8) | .805<br>(.082) | 11.5 | 40.9 |
| Weak | −2.3<br>(4.3) | −1.8<br>(2.5) | −.5<br>(1.8) | .833<br>(.061) | <1 | 43.1 |
| Inspiring | 3.7<br>(4.2) | −.8<br>(2.4) | −.8<br>(1.9) | .938<br>(.083) | 1.4 | 50.8 |
| Provide strong leadership | −1.7<br>(3.7) | 2.8<br>(2.2) | −3.1<br>(1.7) | .903<br>(.073) | 8.5 | 48.3 |
| Knowledgeable | 8.6<br>(3.8) | −1.2<br>(2.2) | −3.3<br>(1.6) | .628<br>(.078) | 8.0 | 38.2 |
| Solve economic problems | 1.0<br>(3.8) | 1.4<br>(2.2) | −2.2<br>(1.7) | .990<br>(.092) | 2.8 | 47.5 |
| Develop good relations | 3.9<br>(4.7) | 2.2<br>(2.7) | −3.2<br>(2.0) | .968<br>(.098) | 7.1 | 50.9 |
| | | | Carter Issue Positions | | | |
| Lib./cons. ideology | 3.6<br>(3.0) | .4<br>(1.8) | −2.6<br>(1.3) | .794<br>(.085) | 2.1 | 42.1 |
| Govt. spending vs. services | −3.9<br>(3.5) | −1.1<br>(2.1) | .9<br>(1.3) | .763<br>(.098) | <1 | 33.2 |

**TABLE B-2**

**Errors-in-Variables Parameter Estimates, September 1980**

| DEPENDENT VARIABLES | TV NEWS EXPOS. | NEWSP. EXPOS. | PARTY ID | FEB. OPIN. | S.E. OF REGR. | $X^2_{(30)}$ |
|---|---|---|---|---|---|---|
| Carter Positions (continued) | | | | | | |
| Defense spending | 1.2 | −.8 | −1.2 | .811 | 4.2 | 33.5 |
| | (3.5) | (2.1) | (1.5) | (.132) | | |
| Relations with Russia | −.5 | 2.9 | −2.9 | .735 | <1 | 48.6 |
| | (4.2) | (2.4) | (1.6) | (.095) | | |
| | Reagan Traits | | | | | |
| Moral | −1.9 | 5.1 | 4.9 | .848 | 8.3 | 30.8 |
| | (3.6) | (2.1) | (1.4) | (.091) | | |
| Dishonest | −3.7 | −4.1 | −4.4 | .829 | 10.5 | 39.4 |
| | (3.9) | (2.2) | (1.5) | (.118) | | |
| Power-hungry | −2.4 | −.7 | −7.0 | .951 | 10.6 | 37.9 |
| | (4.7) | (2.8) | (2.0) | (.088) | | |
| Weak | 8.8 | −5.3 | −3.0 | 1.000 | 11.4 | 31.4 |
| | (4.8) | (2.7) | (1.8) | (.007) | | |
| Inspiring | −8.4 | 1.1 | 4.5 | .772 | 15.1 | 39.8 |
| | (3.9) | (2.3) | (1.6) | (.072) | | |
| Provide strong leadership | −8.3 | −1.3 | 2.4 | 1.000 | 10.3 | 33.7 |
| | (4.3) | (2.5) | (1.6) | (.013) | | |
| Knowledgeable | −8.7 | 1.8 | 4.4 | .818 | 14.6 | 33.9 |
| | (4.1) | (2.3) | (1.7) | (.112) | | |
| Solve economic problems | −9.6 | −1.9 | 4.4 | .826 | 10.5 | 31.6 |
| | (3.6) | (2.1) | (1.8) | (.100) | | |
| Develop good relations | −10.6 | −.1 | 6.3 | .863 | 12.3 | 36.1 |
| | (4.2) | (2.4) | (1.9) | (.102) | | |
| | Reagan Issue Positions | | | | | |
| Lib./cons. ideology | 4.2 | −.4 | 1.5 | .887 | 4.3 | 30.1 |
| | (3.2) | (1.8) | (1.2) | (.070) | | |
| Govt. spending vs. services | −1.3 | −1.7 | 2.3 | .710 | 11.6 | 33.0 |
| | (3.6) | (2.1) | (1.3) | (.121) | | |
| Defense spending | 2.4 | 1.0 | .3 | .701 | 12.8 | 36.0 |
| | (3.5) | (2.0) | (1.3) | (.107) | | |
| Relations with Russia | −5.5 | 2.7 | −3.3 | 1.000 | 5.2 | 39.4 |
| | (3.9) | (2.3) | (1.4) | (.009) | | |

*Note*: Intercepts and parameter estimates for age, education, and race not shown. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.

## APPENDIX C

Table 1 in the text reports parameter estimates from a covariance structure analysis of candidate traits. The aim of that analysis is to refine the estimated media exposure effects in Appendix B by exploiting the availability of multiple indicators of essentially similar trait dimensions.[28]

The estimated factor loadings for the individual trait items are shown in Table C-1. To facilitate comparison between the media exposure effects in Table 1 and those in Appendix B, the factor loadings for each trait dimension in Table C-1 are normalized to have an average absolute value of 1.0. It is clear from the results in Table C-1 that all of the individual trait items are strongly related to the corresponding dimensions in ways that make good substantive sense. Moreover, the patterns of factor loadings are quite similar for the separate analyses of Carter and Reagan traits, except that the leadership dimension had a somewhat stronger impact on responses to the "weak" item for Carter and the "inspiring" item for Reagan. Furthermore (as with the opinion-change models for the individual indicators in Appendix B), these more elaborate dimensional models appear to fit the data well.[29]

**TABLE C-1**

**Measurement Model Parameter Estimates for Carter and Reagan Trait Dimensions**

| TRAIT DIMENSIONS AND COMPONENT ITEMS | CARTER[a] |  | REAGAN[b] |  |
|---|---|---|---|---|
|  | FACTOR LOADING | ITEM-SPECIFIC STD. DEV. | FACTOR LOADING | ITEM-SPECIFIC STD. DEV. |
| **Character** |  |  |  |  |
| Moral | .876<br>(.035) | 16.6 | .951<br>(.030) | 15.3 |
| Dishonest | −.864<br>(.029) | 18.2 | −.899<br>(.029) | 17.2 |
| Power-hungry | −1.260<br>(.041) | 20.1 | −1.151<br>(.038) | 19.7 |
| **Leadership** |  |  |  |  |
| Weak | −.938<br>(.026) | 20.2 | −.628<br>(.032) | 18.8 |
| Inspiring | .965<br>(.022) | 17.6 | 1.245<br>(.031) | 15.0 |
| Provide strong leadership | 1.097<br>(.020) | 15.9 | 1.127<br>(.025) | 15.7 |
| **Competence** |  |  |  |  |
| Knowledgeable | .787<br>(.033) | 19.3 | .825<br>(.031) | 17.5 |
| Solve economic problems | 1.106<br>(.029) | 16.0 | 1.056<br>(.027) | 15.1 |
| Develop good relations | 1.107<br>(.030) | 19.5 | 1.118<br>(.025) | 15.7 |

*Note*: Estimated effects of television news exposure, newspaper exposure, party identification, and prior opinions on latent trait dimensions are reported in Table 1. Asymptotic standard errors of parameter estimates in parentheses. Number of observations = 758.

[a] $\chi^2_{(134)}$ = 297.0, 272.7, and 333.2 for character, leadership, and competence, respectively.

[b] $\chi^2_{(134)}$ = 333.3, 295.9, and 282.3 for character, leadership, and competence, respectively.

## Notes

Previous versions of this paper were presented at Princeton University, Carnegie Mellon University, the University of Chicago, the 1991 Political Methodology summer meeting, and the 1992 annual meeting of the American Political Science Association. I am grateful for stimulating reactions on each of these occasions, and especially to Christopher Achen, Charles Franklin, Shanto Iyengar, Simon Jackman, John Jackson, Norman Nie, and John Zaller for specific suggestions incorporated in this revision.

1. This assumption is consistent with the empirical literature on learning from the media, which suggests that people who are most informed about public affairs are typically most likely to absorb any given piece of new information (Price and Zaller 1992; Robinson and Levy 1986). It is worth noting that if the assumption held exactly, the unconstrained nonlinear model in which $\pi_{ijs}$ and $\pi_{ijt}$ are separate variables with parameters to be estimated would be underidentified; indeed, difficulties encountered in estimating the parameters of the unconstrained nonlinear model may be attributable to the approximate correctness of this simplifying assumption for relatively short-term processes of opinion change. The same simplifying assumption would presumably be less adequate for long-term processes of opinion change, where prior certainty (but not receptivity to new information) may be strongly related to age or experience (Achen n.d.).

2. The most straightforward way to assure that this model is identified is to assume that all the stochastic terms ($\zeta_{it}$, $\delta_{ijs}$, $\delta_{ijt}$, and $\varepsilon_{ijt}$) have mean zero and are uncorrelated with each other and that we have consistent estimates of the measurement error variances $\sigma^2_{\zeta t}$, $\sigma^2_{\delta js}$, and $\sigma^2_{\delta jt}$. These assumptions can be relaxed somewhat when the right sorts of additional data are available, as in the following empirical analysis.

3. The data were originally collected by the Center for Political Studies of the Institute for Social Research, University of Michigan, under the direction of Warren E. Miller. They are available through the Inter-University Consortium for Political and Social Research.

4. These values are too high to be consistent with independent estimates of the size of the audience for network news. Since measurement error models of the sort employed in the following analysis cannot distinguish between stable "true" responses and consistent overreporting of news exposure, the parameter estimates for news exposure effects (but not the estimates of aggregate exposure effects) will be artificially attenuated by the inclusion of some overreporters among those apparently exposed to the news.

5. The estimated effect of February newspaper exposure on June newspaper exposure (in an equation including age, education, race, and party identification as control variables and adjusting for measurement error) is .927 (with a standard error of .052). The estimated effect of June newspaper exposure on postelection newspaper exposure in an equation including the same control variables, in spite of the slight change in question wording and some panel attrition, is .769 (with a standard error of .041). The average effect of June newspaper exposure on September opinions in the analysis that follows is 20% greater than the average effect of June newspaper exposure on June opinions. This difference parallels the 18% increase in the average effect of television news exposure from June to September, suggesting again that June newspaper exposure is an adequate proxy for September newspaper exposure.

6. It would be desirable from a theoretical standpoint to

allow for the possibility that party identification conditions the impact of media exposure (via selective perception) in addition to mattering in its own right. I am convinced, on the basis of some exploratory analysis, that partisanship and media exposure do interact significantly; but it is impossible to pursue that interaction rigorously here, given the other complexities of the statistical model and the limitations of the available data.

7. The estimated reliability of measured party identification is .88.

8. Given the logic of Bayesian updating, the stability coefficients for all variables should be less than or equal to 1.000. That constraint is binding for the estimated stability coefficients for party identification in the following analysis, but only barely: the corresponding unconstrained estimates (from a model including age, education, and race as control variables) are 1.008 (with a standard error of .026) from February to June and 1.005 (with a standard error of .021) from June to September.

9. The estimated effects of these demographic variables on television news exposure in the first wave of the panel (with standard errors in parentheses) are as follows: age (in years), .0050 (.0007); education (in years), −.001 (.004); black, .052 (.039); party identification, −.035 (.019). The estimated effects of these variables on newspaper exposure in the first wave of the panel are as follows: age, .0067 (.0010); education, .049 (.006); black, .100 (.059); and party identification, .001 (.029).

10. The parameter estimates associated with these control variables in the opinion change equations are omitted from the tables due to space constraints but are available from the author. It is, of course, conceivable that media exposure could be correlated with unmeasured causes of opinion change even after controlling for party identification, age, race, and education. Unfortunately, there is no obvious way to address this remaining potential endogeneity, since any available instrument for media exposure might itself be a direct cause of opinion change. Fortunately, potential biases of this sort seem unlikely to be very important, at least if we interpret the estimated media exposure effects broadly (as including the effects of correlated exposure to other campaign media), rather than narrowly (as specific effects of exposure to network television news and daily newspapers).

11. Issue positions were recoded so that 0 denotes the liberal endpoint and 100 the conservative endpoint of each issue scale.

12. The standard Wiley–Wiley measurement model is augmented here to make ("true") television news exposure in each wave of the panel a function of age, education, race, and party identification in addition to previous television news exposure, and likewise for newspaper exposure. In addition, disturbances for the television news and newspaper exposure equations are allowed to be correlated (though the correlations turn out to be small—.03 in June and −.13 in September).

13. With three waves of panel data for a single variable, the basic Wiley–Wiley model is just identified. Hence it is impossible to test its goodness of fit. Here, the availability of additional data makes it possible to test the goodness of fit of the model; and in every case, the fit is quite good. It is also possible to relax the conventional assumptions somewhat—for example, by allowing measurement error variances to differ across panel waves or by allowing measurement errors for different responses by the same respondent to be correlated. Having explored several modifications of this sort, I found none that produced more than marginal improvements in the statistical fit of the model and none that appreciably changed the substantive results. For example, allowing measurement errors for thermometer ratings of Jimmy Carter and Ronald Reagan to be correlated produces an estimated format-induced correlation of .20; but the average difference in the eight estimated media exposure effects resulting from this generalization of the model is only .12. For additional examples of alternative model specifications (again, with no appreciable impact on the substantive results of the analysis), see nn. 12, 28. These results are consistent with those reported by Feldman, who applied the Wiley–Wiley model to a variety of items similar to those used here (party identification, issue positions, and candidate evaluations) using data from a five-wave panel (for which the model was overidentified) and concluded that "the simple measurement model fits very well" (1989, 33, 38).

14. All of the parameter estimates that I report were produced using the generalized least squares routine in the EQS software package (Bentler 1989).

15. This classification is imposed purely for descriptive purposes in Figures 1–4 but reappears as the basis for a more explicit dimensional analysis in Table 1.

16. Some perspective on the magnitude of these effects may be gained by noting that in a probit analysis of vote choices, a three-point change in the thermometer rating for either candidate in the last month of the campaign translated into a corresponding change of up to 5 or 6 percentage points in the probability of actually voting for that candidate. Vote choices are not analyzed directly here, because prospective voters were not asked about their vote intentions until the third (September) wave of the 1980 panel.

17. Any comparison of television news and newspaper exposure effects must recognize that many people may "read a daily newspaper regularly" for sports news, horoscopes, and want ads, without paying the least attention to news of national politics and public affairs of the sort primarily featured on the television network news programs. A more precise measure of exposure to a daily newspaper's *political* news would produce somewhat larger exposure effects but a correspondingly lower estimate of how many people are exposed. Thus, although the differences reported here in estimated exposure effects should not be taken as evidence that print news is inherently less persuasive than broadcast news, they do demonstrate that it is less influential in the aggregate.

18. The individual ordinary least squares parameter estimates summarized in Figures 1–4 are available from the author.

19. The average standard errors (estimates of $\sigma_u$ from equation 3) for the 37 separate opinion regressions on the common 100-point scale are 10.5 in June and 6.9 in September. The average of the 37 distinct chi-squared values is 41.9—of which 19.0 is attributable to the basic exposure model relating media exposure in each wave of the panel to prior media exposure and demographic variables (with 12 degrees of freedom) and 22.9 is the average increment attributable to the model in equation 3 relating opinions in each wave of the panel to prior opinions, media exposure, and demographic variables (with 18 degrees of freedom). In each of the 37 models, the Bentler Comparative Fit Index is 1.000 (1989, 114–17).

20. From successive application of equation 1,

$$\theta_{ij3} = [\theta_{ij1}\lambda_{j2} + \mu_{ij2}(1 - \lambda_{j2})]\lambda_{j3} + \mu_{ij3}(1 - \lambda_{j3})$$

$$= \theta_{ij1}\lambda_{j2}\lambda_{j3} + \mu_{ij2}(1 - \lambda_{j2})\lambda_{j3} + \mu_{ij3}(1 - \lambda_{j3}).$$

Thus, the combined weight attached to the campaign information terms $\mu_{ij2}$ and $\mu_{ij3}$ is

$$(1 - \lambda_{j2})\lambda_{j3} + (1 - \lambda_{j3}) = 1 - \lambda_{j2}\lambda_{j3}.$$

The "campaign information" estimates in Table 2 were calculated by replacing $\lambda_{j2}$ and $\lambda_{j3}$ in this expression with the corresponding errors-in-variables and ordinary least squares parameter estimates.

21. In the case of party identification, the entries in Table 2 represent the differences between the messages received by Strong Republicans and Pure Independents (or between the messages received by Pure Independents and Strong Democrats).

22. Since the $\boldsymbol{\beta}_{jt}$ parameters in equation 3 are defined in terms of the $\boldsymbol{\alpha}_{jt}$ and $\lambda_{jt}$ parameters as $\boldsymbol{\beta}_{jt} = \boldsymbol{\alpha}_{jt}(1 - \lambda_{jt})$, estimates of the $\boldsymbol{\alpha}_{jt}$ parameters for each variable in each panel wave can, in principle, be retrieved by substituting esti-

mates of the $\beta_{jt}$ and $\lambda_{jt}$ parameters into the equation $\alpha_{jt} = \beta_{jt}/(1 - \lambda_{jt})$. However, this approach fails for 6 of the 74 possible separate calculations—because the estimated lag parameter $\lambda_{jt}$ equals the theoretical maximum value of 1.00 (so that the estimated denominator of the ratio $\beta_{jt}/(1 - \lambda_{jt})$ is zero)—and provides very untrustworthy estimates in several other cases because the estimated denominator is close to zero. This problem can be mitigated by cumulating over campaign periods, as with the estimated relative weight of campaign information reported in Table 2, col. 1. Since the cumulative campaign message is simply the precision-weighted average of the separate messages $\mu_{ij2}$ and $\mu_{ij3}$—with precisions $(1 - \lambda_{j2})\lambda_{j3}$ and $(1 - \lambda_{j3})$, respectively—the contribution of characteristic j to the cumulative campaign message is the same precision-weighted average of the separate message parameters $\alpha_{j2}$ and $\alpha_{j3}$,

$$[\alpha_{j2}(1 - \lambda_{j2})\lambda_{j3} + \alpha_{j3}(1 - \lambda_{j3})]/[(1 - \lambda_{j2})\lambda_{j3} + (1 - \lambda_{j3})].$$

Substituting $\beta_{j2}/(1 - \lambda_{j2}) = \alpha_{j2}$ and $\beta_{j3}/(1 - \lambda_{j3}) = \alpha_{j3}$ into this expression, the contribution of characteristic j to the cumulative campaign message can be expressed in terms of the $\beta_{jt}$ and $\lambda_{jt}$ parameters as $(\beta_{j2}\lambda_{j3} + \beta_{j3})/(1 - \lambda_{j2}\lambda_{j3})$. Substituting the corresponding errors-in-variables or ordinary least squares parameter estimates for $\beta_{j2}$, $\lambda_{j2}$, $\beta_{j3}$, and $\lambda_{j3}$ in this ratio produces the estimated contributions reported in Table 2 of television news exposure, newspaper exposure, and party identification to the cumulative campaign message for each of the 37 separate opinions.

23. Here, as elsewhere, it may be worth noting that the effects of party identification, though large for individual identifiers, are much smaller for the population as a whole, simply because effects on Republicans and opposing effects on Democrats tend to cancel out. Even if every strong partisan absorbed new information 15 or 20 points more favorable toward his or her own candidate than the new information absorbed by "pure" independents, as the results in Table 2 suggest, the corresponding average *aggregate* impact of this partisan reinforcement would have been only two points or so on the 100-point scale, given the distribution of party identification in 1980. By contrast, the average aggregate impact of television news exposure on the messages received by the population as a whole is on the order of 6 points, and the average aggregate impact of newspaper exposure is on the order of 3 points on the 100-point scale.

24. The distributions of $\alpha_{jt} = \beta_{jt}/(1 - \lambda_{jt})$ shown in Figures 5 and 6 are based on samples of 10,000 pseudo observations each for $\beta_{jt}$ and $\lambda_{jt}$. The pseudo observations were sampled (from Normal distributions with means, variances, and covariances derived from the empirical analyses reported in Appendix B and, in Figure 5, from the corresponding ordinary least squares analysis) and the distributions constructed by Simon Jackman.

25. There are many cases in which February values of the variables examined here are significantly related to media exposure, but the difficulties involved in making causal inferences from such cross-sectional relationships are daunting.

26. Patterson's (1980) 1976 election year panel study may be helpful in this regard, since it includes a somewhat more extensive battery of media exposure questions than the National Election Study surveys used here. Patterson also had the good fortune to be surveying a public with very weak preexisting opinions about one of the eventual nominees, Jimmy Carter.

27. "That's how it goes: One person believes that I'm the ceremonial gypsy goddess human sacrifice of a Mexican death cult, and soon everyone else just knows it, too—even though they don't even go to the supermarket" (Barr 1989).

28. In addition, the availability of multiple indicators of each general dimension makes it possible to further relax the assumptions of the measurement model applied to the individual indicators. The estimates presented in Tables 1 and C-1 are from a model in which measurement errors for different measures of the same image dimension are allowed to be contemporaneously correlated. The 18 estimated measurement error correlations (three for each of the three-image dimensions for each of the two candidates) range from −.07 to .18 and average .06. These correlations are large enough to improve the fit of the model slightly but do not significantly affect the results. Additional experimentation with model specification suggests that further weakening the measurement assumptions (e.g., allowing measurement error variances and covariances to vary across waves of the panel) would also improve the fit of the model somewhat, without significantly affecting the results reported here.

29. The average value of the Bentler Comparative Fit Index for the six separate models (one for each candidate for each trait dimension) is .998, and the average chi-squared value is 302.4 (with 134 degrees of freedom). The average standard error for the 12 distinct opinion regressions is 8.0 on the 100-point scale, as compared with an average standard error of 9.6 for the 36 corresponding individual item regressions in Appendix B.

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# NEIGHBORHOOD POVERTY AND AFRICAN AMERICAN POLITICS

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*William Wilson and other scholars argue that one of the attributes of devastated neighborhoods is social isolation. We shall explore whether neighborhoods that seem to indicate significant social isolation also foster political isolation. We begin our examination by providing a description of the poor in the samples from the 1989 Detroit Area Study. We then turn our attention toward analyzing the effects of neighborhood poverty on African-American public opinion and political participation. We conclude with a discussion of how neighborhood poverty affects African-American politics and the consequences of those politics for the theory and practice of American democracy.*

> And to this I say, that every man that hath any possession, or enjoyment, of any part of the dominions of any government, doth thereby give his tacit consent, and is as far forth obliged to obedience to the laws of that government, during such enjoyment, as any one under it; whether this his possession be of land, to him and his heirs for ever, or a lodging only for a week; or whether it be barely travelling freely on the highway; and in effect, it reaches as far as the very being of anyone within the territories of that government.
>
> John Locke, *Second Treatise of Government*

> Man how could you think you're an American when you haven't ever had any kind of an American treat over here? You have never, never. Ten men can be sitting at a table eating, you know, dining, and I can come and sit down where they're dining. They're dining; I've got a plate in front of me, but nothing is on it. Because all of us are sitting at the same table, are all of us diners? I'm not a diner until you let me dine. . . . Just because you're in this country doesn't make you an American. No, you've got to go farther than that before you can become an American. You've got to enjoy the fruits of Americanism. You haven't enjoyed the fruits. You've enjoyed the thorns. You've enjoyed the thistles. But you have not enjoyed the fruits, no sir.
>
> Malcolm X, *Malcolm X Speaks*

Several scholars point out that poverty, especially as it affects African Americans and other minorities, is once again gaining recognition as a major American problem.[1] The effect of poverty on the American economy, "fairness" to the middle class, political stability, and urban futures, as well as the human cost of the transformation of inner cities into concentrations of poverty, is once again being discussed. While in the 1960s, "liberal" arguments dominated academic and policy debates on poverty, emphasizing the government's responsibility toward such individuals and communities, in the current era "conservative" arguments focusing on the impact of urban poverty on the moral fabric of American came to dominate the public's consciousness during the 1980s (Lemann 1986; Mead 1986; Murray 1984). Discussions of urban poverty have skyrocketed to the top of both academic and funding agendas. And as might be expected, much of the scholarly attention on poverty has focused either on detailing and counting the "pathologies" of urban poverty that "threaten" nonimpoverished individuals and communities or has centered on tracing the patterns and presumed causes of poverty, many of which are sporadically highlighted on the evening news (Levy 1980; McLanahan 1983).

However, in spite of the ever-growing literature on the urban poor and the "underclass," generally little attention has been paid to the impact of poverty on the political behavior and opinion of individuals. Political scientists, in particular, have neglected the one area that could lead to the empowerment and involvement of poor individuals in struggles and strategies to change their status, namely, politics (as distinguished from policy formation). Research exploring the effects of economic stress on the political attitudes of the poor is one example of an attempt to detail the political consequences of poverty, but work of this nature is far too rare (Rosenstone 1982; Schlozman and Verba 1979).

Apart from the lack of questions concerning the relationship between poverty and political behavior, traditional poverty research has also tended to focus on individual poverty. As might be expected, much of the recent poverty research directing attention to the impact of poverty on individuals' existence has focused on the creation of "social pathologies," emphasizing the connection between an impoverished existence and poverty's destructive impact on how people think and act. Only recently has the concentration of poverty within a given neighborhood been explored as a factor contributing, in and of itself, to the devastation of poverty. Significantly less attention has been paid to the impact of concentrated poverty in "deadly neighborhoods" on an individual's ability to function as a participatory citizen.[2]

In this study, we examine the impact of neighborhood poverty on the political attitudes and behavior of African Americans, beyond the consequences of individual poverty. Specifically, we test hypotheses advanced by scholars such as Marable (1983, 1992) and Wilson (1987, 1991) suggesting that neighborhoods engulfed by the severest poverty are qualitatively different, in the structural opportunities, both political and economic, that they afford their residents, from neighborhoods without extreme concentrations of poverty. Wilson (1991) argues that the "concentration effects" of extremely poor neighborhoods provide a social context or environment that fosters weak labor force attachment, crime, and the other phenomena that plague the most economically devastated segments of society. A consequence of these neighborhoods, Wilson argues, is social isolation, where residents of these communities become disconnected from the community or group structures and networks that facilitate their economic and social participation. Thus, one would expect alienation and isolation to become dominant characteristics of the poorest neighborhoods.

Marable also contemplates the relationship between economic stress and opportunities and organization for poor communities. He argues that in the context of decreasing federal and economic support, many inner cities dominated by African Americans are experiencing internal crises where those at the bottom of the economic ladder (the black poor for Marable) have become disengaged from the indigenous institutions and networks of community support, as well as from the larger political system (Marable 1984). Marable's theoretical analysis of the political and social organization of the black poor is particularly grim. Marable argues that extreme black poverty leads to social disorganization. Aspects of an impoverished existence, such as fear due to crime and other debilitating features of inner city life, serve to inhibit the formation of black consciousness and foster "fragmentation and collective doubt" concerning the evaluations of the social and political progress of African Americans. Under such conditions, we would again expect the African American poor, especially those living in persistently impoverished neighborhoods, to exhibit less efficacy and affect toward other African Americans.

We believe it important to pay attention to such scholars as Marable, who (although often not recognized as a full participant in the mainstream poverty debates) represents the mass of scholars concentrating on issues of race and ethnicity, who, because of the social and economic marginalization of those communities they study, find themselves obliged to explore the effects of poverty when examining the political behavior of group members.[3]

Through this analysis, we build upon Wilson and Marable's analyses, examining how different neighborhoods produce political environments, which, in turn, structure African American political choice. We extend our analysis of the cumulative environmental effects of extremely poor neighborhoods to the realm of African American public opinion and political behavior. Just as Wilson argues that poor neighborhoods foster weak labor force attachment, we investigate whether poor neighborhoods foster weak attachment to the political system as manifested in the beliefs and behavior of residents of those neighborhoods; as Wilson argues that one of the attributes of devastated neighborhoods is social isolation, we explore whether these neighborhoods also foster political isolation. Further, if Marable was correct in concluding that poverty inhibited the formation of group consciousness through which different members of the African American community developed a shared worldview, to what degree does poverty exacerbate political stratification in the black community?

Finally, apart from the examination of statistical differences in the political behavior of African Americans from varying social contexts, we believe that the study of neighborhood poverty presents a number of challenges not only to students concerned with the immediate impact of poverty on African American politics but also to students of the theory and practice of American democracy. We argue that neighborhoods are termed "deadly" not only because the poor conditions of these neighborhoods threaten their inhabitants with significant physical and psychological harm and contribute to the weakening of their residents' attachment to the labor force but also, critically, because these neighborhoods do not provide the fundamental requirements necessary for the functioning of democracy. Rousseau (1978) argues in *The Social Contract* that without equality, freedom cannot last. He then makes his famous argument: "With regard to wealth, no citizen shall ever be so opulent that he can buy another, and none so poor that he is constrained to sell himself" (p. 75.)

Many theorists, ranging from Rousseau to Malcolm X, have argued that the foundations of a good society—in particular a democratic society—must include the basics of economic and physical security for all citizens. What is "democratic" about an environment that causes a weakening of attachment not only to the labor market but also to the political system, decreasing the possibility of citizens' engaging in political actions and decisions? What is "democratic" about a neighborhood where the state is unresponsive, in all but its most coercive aspects, to the needs and preferences of the residents of that neighborhood? To what degree does such deprivation in physical and economic security challenge the assumption of consent among these citizens?

Modern and classical theorists have argued that consent, legitimacy, and obligation (inherent in a functioning democratic system) depend on (1) absence of "a setting so threatening and volatile that one is always reacting" (Herzog 1989, 192) and (2) a minimum amount of governmental responsiveness to citizens (Pateman 1988). As Herzog states, "Any plausible account of legitimacy and obligation must center on whether the state is for the most part responsive to the people" (1989, 205). Democratic theorists have also claimed that protection of citizens by the state is an essential feature of the social contract through which citizens become obligated to the state (Pateman 1988). However, we question to what degree the state protects citizens in neighborhoods where over half of the children are living in poverty, where infant mortality rates equal or exceed those in parts of the Third World, where large percentages of residents have been unemployed for extended periods of time, and where citizens suffer from the threat of high crime rates and consistent violence.

Some democratic theorists have rejected conceptualizations of citizenship and obligations based on a fictional Lockean or Hegelian neutral state. They argue that the American state and its policies are instrumental in creating and reproducing patterns of disadvantage dependent on the exploitation of economic resources, gender, and race (Young 1989, 1990). Further, many of these same scholars argue that the myth of a "neutral and impartial" state systematically privileges in the political arena those who are white and affluent (Young 1989). The institutionalization of disadvantage centered around the intersection of race and poverty in the neighborhoods that are our subject places residents at a severe disadvantage not only in their ability to function as economic citizens but in their political roles, as well. We argue that the deadly conditions of the most devastated neighborhoods of the inner city, where political legitimacy, attachment, and governmental responsiveness are lacking, provide one of the most glaring failures of the democratic process in America. The continued and exacerbated breakdown of democracy in these neighborhoods, we and others argue, has led, and will lead, to greater levels of political distance between African Americans in these neighborhoods and African Americans in less impoverished communities, as well as those in the greater population.

Within this more general framework, we begin our examination by providing a description of the poor in the samples from the 1989 Detroit Area Study. We then turn our attention toward analyzing the effects of neighborhood poverty on African American public opinion and political participation. We conclude with a discussion of how neighborhood poverty affects African American politics and the consequences of those politics for the theory and practice of American democracy.

## DATA, METHODOLOGY, AND MODELS

To examine these questions, we use the 1989 Detroit Area Study.[4] The 1989 Detroit Area Study is comprised of 916 face-to-face interviews from a multistage random sample of private households in the Detroit tricounty area. The core sample included 466 respondents and was supplemented with an oversample in Detroit of an additional 450 respondents. There are 448 African Americans in our sample. The individual-level data in the sample were supplemented with data from the 1980 census with census tract information matched to each respondent.

Throughout this analysis we use 1980 census tract data to approximate the impact of neighborhood poverty. We clearly recognize the limitations of such an approach but argue that census tract data offer the best approximation available to us at this time.[5] Understanding the limitations of this measurement, we do not contend that our findings are to be seen as the complete story on neighborhood poverty effects but, instead, as a first step in the ongoing process of understanding the direct and indirect impact of poverty on political behavior. Further, we argue that if, in fact, we do find effects with such a "crude" measurement, then we should interpret this as a positive indicator of real underlying effects. We also want to acknowledge many scholars concerns about contextual measures—specifically, specification error, nonlinear effects, and spillover from misspecified individual-level data (Ebring and Young 1979; Giles and Dantico 1982; Hauser 1974). We believe all of these concerns point to the need for careful development of our constructs. Thus, we have included individual-level measures that are standards in the behavioral field and aggregate-level measures that ensure variation across census tracts. Finally, in future research projects, we hope to use 1990 census data; however, since tracts tend to change rather slowly, there is no reason to believe that the 1980 census tract data misrepresent current patterns of poverty.

### Neighborhood Poverty in Detroit

Unlike the cities studied by other researchers interested in neighborhood poverty and politics, Detroit is a city situated in the heart of the geographical area that Wilson identifies as the sole region where the "underclass" grew between 1970 and 1980 (Berry, Portney, and Thomson 1991; Wilson 1991).[6] Indeed, Detroit is one of the five cities accounting for two-thirds of all U.S. growth between 1970 and 1980 in persistently poor neighborhoods (Wilson 1991).[7] Again, we use the 1989 Detroit Area Study to examine the impact of poverty on the public opinion of African Americans as well as on their political participation, measuring poverty both as individual poverty (household income) and as neighborhood or contextual poverty (percentage of households in a census tract below the poverty line). As mentioned, we are interested in seeing whether theories of social isolation translate into similar patterns of alienation and difference in the political arena. Does increasing neighborhood poverty negatively impinge on the ability of residents to participate in and perceive influence in political decision making? Do high levels of neighborhood poverty encourage African American detachment from the political system?

Patterns of spatial concentration of poverty can be clearly identified within the context of poverty in Detroit.[8] Despite the reputation Detroit has gained as one of the most economically devastated cities (Darden et al. 1987), few African American households in Detroit (5%) are found to live in areas where there exist extreme concentrations of poverty as described by Wilson (1987). Wilson argues that extreme neighborhood poverty is found in neighborhoods where at least 40% of the households exist economically below the poverty line. And while 40% neighborhood poverty is not the norm for black Detroiters, a less extreme but no less daunting picture of poverty still presides in the city. Over one-third—nearly 37%—of all African American households in Detroit have incomes below ten-thousand dollars. Further, our data indicate that 22% of all African American households in Detroit, independent of personal income, are located in neighborhoods where at least 20% of the households in the area have incomes below the poverty line (Detroit Area Study 1989). However, as might be expected, most of the economically disadvantaged black households in Detroit (nearly 65% of those existing below the poverty line) are concentrated in neighborhoods that have 20% or more of contextual poverty (households with incomes below the poverty line). Although this distribution of neighborhood poverty is not as concentrated as Wilson might predict, high levels of African American neighborhood poverty are all too prevalent in Detroit.

Throughout our analysis, we examine the impact of four levels of neighborhood poverty, measuring neighborhood poverty as the percentage of households in a census tract below the poverty line. The four measures of contextual poverty used in this analysis are defined as follows: (1) those respondents living in neighborhoods with little or no contextual poverty, 0–10%; (2) neighborhoods with 11–20% contextual poverty; (3) neighborhoods with 21–30% poverty; and (4) neighborhoods with at least 31% contextual poverty.[9] Although we include no separate category mirroring Wilson's defined category of *extreme contextual poverty* (40% and above), we feel that our last category sufficiently reflects those conditions of extreme poverty as articulated by Wilson and other researchers.[10] Further, the inclusion of our last category is especially important because much of the recent work on contextual poverty has been unable to test the significance of such extreme conditions due to the limitations of the data sets used in these analyses (Berry, Portney, and Thomson 1991). We believe that most of the political differences generated by environmental structure will appear within the African American community in the extreme category of 31%.

### Models and Methodology

For this investigation, we developed a general model to explore systematically the effects of neighborhood poverty on African American public opinion and political behavior. This analysis focused on observing the causal effects, both indirect and direct, of neighborhood poverty on politics. Specifically, we were interested in testing the proposition that there exists a causal relationship between levels of neighborhood poverty and the political behavior and public opinion of African Americans.[11] Our goal in this project, therefore, was not to specify an "exact" model of participation or public opinion but, instead, to examine the relationship between neighborhood poverty and questions of political behavior.[12]

In our model, we attempted to look for causal relationships, holding other demographic and socioeconomic characteristics constant. We included in our equations measures of socioeconomic status known to affect public opinion and political participation. The independent variables used in the model other than measures of contextual poverty included age, individual poverty status, education, the log of family income, and gender (see Appendix A).[13] In particular, we sought to control for a variety of individual-level socioeconomic variables in order to guarantee that we were generating reliable estimates of the effect of neighborhood poverty. The four levels of contextual poverty were used as dummy variables in our equations, with three directly represented in the equations. The effects of 0–10% contextual poverty were understood to be included in the constant. We use these dummy variables of neighborhood poverty to identify easily possible threshold effects of contextual poverty. Specifically, do all levels of neighborhood poverty have varying degrees of impact on political behavior; or is the impact of poverty more accurately represented by a step function, where at certain threshold levels the effects of poverty are truly evident? This general model was estimated with ordinary least squares procedures except in those cases where the dependent variable was coded as a dichotomous variable. In these instances, probit estimation was used. We now turn to the analysis of the effects of neighborhood poverty on African American politics.

## SOCIAL ISOLATION AND NEIGHBORHOOD POVERTY

We believe that social isolation, as reflected in resource opportunity structure, plays a major role in shaping the politics of those who reside in the poorest neighborhoods. Over the years, the concept of social isolation, especially as it is associated with the African American poor, has come to embody a number of meanings. Most recently, social scientists talk of the social isolation of the black poor, in response to a perceived exodus of more affluent and middle-class blacks from central cities or certain poor neighborhoods in urban cities for greater opportunities in surrounding areas and neighborhoods. This line of analysis contends that as working-class and middle-class blacks departed from "traditional" black neighborhoods, so too, did social, political, and economic networks that were so important in connecting the interests of those in poor neighborhoods to the progress of their middle-class sisters and brothers.

Both of the present authors have argued that African American social institutions and networks are critical elements in providing an information nexus through which African American perceptions of racial group interests are framed (Cohen, 1993; Dawson n.d.; see also Jackson 1989).[14] It is through these networks that the intergenerational transmission of African American political values, mores, and beliefs occurs. The black church, in particular, remains one of the most critical formal institutions for the transmission and shaping of African American public opinion (Dawson n.d.; Dawson, Brown, and Allen 1990; Henry 1990). Other social groups, such as black fraternal, professional, community, and racial organizations play similar roles. Thus, to the degree that Wilson and other scholars are correct in arguing that the African American poor reside in neighborhoods distinct from the black working, middle, and upper classes, then we can expect to find observable differences in their political opinions. Further, even if these institutions are not physically absent from poor neighborhoods, they may be patronized primarily by middle-class African Americans who no longer live in the community but, for example, come in only on Sunday to go to church.

Separately, Marable (1983) has argued that the level of community destabilization, as evident in rates of crime, increased drug use, and the waning of black social institutions in highly impoverished neighborhoods breaks down feelings of racial group solidarity and community efficacy. This second crisis in group unity, as Marable (1992) describes it, results not only from the immediate social problems of these neighborhoods but also from the "collapse of the economic infrastructure in America's urban centers, and the failure of the Federal government to initiate an economic reconstruction program"; he continues:

> This second crisis can be characterized by a deep sense of fragmentation and collective doubt, rooted in group consciousness and social existence. The symptoms of this internal crisis were the widespread drug epidemic, black-against-black violence, the growth of urban youth gangs, and the destruction of black social institutions. For young African-American males in their twenties, the murder rate was more than one in twenty. Such violence inevitably spread to the character of social relationships. People concerned with street violence, the danger of robbery or death, would be reluctant to attend neighborhood political meetings after dark. Black-owned business in the central cities lost patrons and support making it even harder for small entrepreneurs to survive. . . . Large sections of major cities such as Detroit were depopulated . . . Much of this internal crisis within contemporary black life must be understood against the backdrop of corporate and governmental decisions (pp. 9–10)

Again, once group solidarity is destabilized, we can expect that the networks of communication and community learning, so prevalent in the African American community, will no longer effectively facilitate shared worldviews, norms, and attitudes. Therefore, we expect from Marable's assumptions that the more we find African American poor neighborhoods alienated, isolated, and neglected, the more the patterns of political belief and behavior among residents will differ from other parts of the black community.

Finally, Granovetter (1973) makes the general argument that secondary ties—less intimate networks beyond family and neighborhood—within a community greatly encourages the flow of information through a community.[15] He suggests that strong primary ties lead to highly cohesive but isolated clusters within a given community, often resulting in a fragmented community. On the other hand, loose secondary ties serve as bridges between clusters and allow for greater information diffusion within a community. Thus, the presence of these loose ties within the black community is posited to facilitate both the homogeneity of African American politics, as well as possible shared political mobilization (Dawson 1986; Dawson n.d.; Granovetter 1973). Again, as loose ties are eliminated through social isolation, we can expect African Americans in different community contexts to demonstrate different political views and behavior.

We embrace a broader conception of social isolation, one that recognizes the diminishing ties between middle-class blacks and poor African Americans as a contributing factor in the pervasiveness of poor black people's economic, political, and social exclusion. However, we also recognize and see as primary the role of federal policies (especially decisions about highway construction, incentives for economic development out of the city, policies reinforcing segregated patterns of housing development, lack of support for public education, and the general dearth of attention to, and funding of, cities) as contributing factors in the creation of isolated communities among working-class and increasingly poor blacks. Finally, we want to be clear that when we speak of social isolation, we focus our arguments on factors such as the absence of specific institutions and social groups within a community, rejecting conceptions of isolation that seek to highlight cultural patterns of behavior.

We shall explore the relationship between poverty and social organizations, paying special attention to the general premise of social isolation of impoverished communities. Generally, we examine how neighborhood poverty affects individual black Detroiters' attachment to organizations within the black community and their conceptualization of crime, as well as their ties to politically important social networks. Specifically, we probe measures of the effects of neighborhood poverty on black Detroiter's ties to churches and groups and to public officials, their perception of crime as a serious problem in their own communities, and their probability of being a crime victim (see Appendix B). We apply our basic model to these analyses. The more we find that neighborhood poverty predicts less attachment to these black institutions and social networks, the more we should later expect to find differences in the political opinion and behavior of the residents of devastated neighborhoods. The results, reported in Table 1, clearly indicate that neighborhood poverty has a devastating effect on politically important indicators of social isolation, even after controlling for individual characteristics *including individual poverty*. Further, the results reported in Table 1 display a pattern that tends to confirm Wilson's hypothesis of a threshold effect.

**TABLE 1**

**Neighborhood Poverty and Politically Relevant Indicators of Social Isolation**

| INDEPENDENT VARIABLES | DEPENDENT VARIABLES | | | | |
|---|---|---|---|---|---|
| | CHURCH MEMBER | GROUP MEMBER | KNOWS INFLUENT. PERS. 2D HAND | CRIME VICTIM | THINKS NEIGHB. CRIME SERIOUS |
| Log income | −.08 | −.05 | .10 | .16 | −.06 |
| | (.13) | (.15) | (.14) | (.15) | (.04) |
| Age | 1.28 | 2.01 | .16 | −.08 | .08 |
| | (.30)*** | (.35)*** | (.31) | (.34) | (.09) |
| Education | .52 | 1.93 | 1.95 | .25 | .06 |
| | (.36)* | (.44)*** | (.4)*** | (.39) | (.11) |
| Indiv. poverty | −.17 | −.26 | .16 | .02 | −.15 |
| | (.27) | (.30) | (.28) | (.03) | (.08)** |
| Gender (woman) | .42 | .07 | −.30 | −.29 | −.04 |
| | (.15)*** | (.16) | (.15)*** | (.16)** | (.04) |
| Context. pov. 11–20% | −.26 | −.15 | .44 | .30 | .08 |
| | (.20) | (.23) | (.21)*** | (.22)* | (.06) |
| Context. pov. 21–30% | −.16 | −.17 | .18 | .28 | .15 |
| | (.18) | (.19) | (.18) | (.20) | (.05)*** |
| Context. pov. 31+% | −.67 | −.62 | −.67 | .41 | .44 |
| | (.24)*** | (.24)*** | (.25)*** | (.26)* | (.07)*** |
| Constant | 4.42 | 4.28 | 3.87 | 3.67 | .48 |
| | (.49)*** | (.55)*** | (.52)*** | (.55)*** | (.14)*** |
| N | 346 | 344 | 344 | 345 | 345 |

*Source*: Detroit Area Study 1989.
*Note*: Entries except column 5 are probit estimates followed by the standard error in parentheses. The unstandardized estimates in column 5 are derived using ordinary least squares.
*** $p \leq .025$, one-tailed test.
**$p \leq .05$, one-tailed test.
*$p \leq .10$, one-tailed test.

In other words, the neighborhoods with the very highest concentration of poverty (above 30%) are particularly effective in restricting the social and networking opportunities of African Americans living in them. These neighborhoods often either demonstrate stronger effects on social isolation variables of political interest than other neighborhoods or (in many cases) are alone in exhibiting *any* contextual poverty effect. As we argued earlier, the work of scholars such as Cohen (1993), Dawson (n.d.), Morris (1984), and others highlight the political importance of black community institutions.

The results indicate that living in a neighborhood with over 30% poverty has a chilling or isolating effect on the organizations to which a respondent belongs. While neighborhood poverty was found to have no effect on the frequency of attending church service, neighborhood poverty over 30% significantly lowers one's probability of belonging to either a church organization or, more generally, belonging to any group at all. So, while living in a neighborhood with levels of poverty up to 30% does not seem to lower the probability of African Americans' having ties to key institutions and groups, living in a neighborhood with over 30% poverty does decreases one's probability of belonging to a church organization or community group by the substantial margin of at least 20%.[16] Thus, living in a neighborhood with high levels of economic devastation leads to greater isolation from social institutions that are most involved in black politics, such as the black church and organizations dedicated to racial affairs.

The likelihood of believing that crime is a serious problem and of actually being a victim of crime is, unsurprisingly, also directly affected by the level of poverty in a neighborhood. As expected in both cases, the neighborhood effects are largest in the most economically devastated communities. The perception of living in a neighborhood with serious crime rapidly rises as one moves past the weak differences found in the middle-level concentration of poverty to the top category. There is a similar (if not as dramatic) increase across the thresholds for crime victimization. At the highest level of poverty, the probability of one's viewing crime as a serious problem rises by 44%, and the probability of being a crime victim increases by 13% over those living in neighborhoods with 0–10% poverty. Therefore, if Marable's hypothesis is correct, the perceived higher level of crime in these neighborhoods should also serve to isolate these residents politically from other black neighborhoods, inhibiting their opportunities to participate in actions to meet the needs of their community.

Finally, one's degree of indirect contact with public officials is substantially lowered by living in the poorest neighborhoods. When we examined the impact of neighborhood poverty on knowing a public official, we found no effect. Such a finding suggests that few people, independent of their income or neighborhood, actually *know* a public official. However, when respondents where asked if they knew someone who knew a public official, neighborhood effects were identifiable. What is particularly striking about this finding is that between the second lowest (relative to the constant), and the highest, level of poverty, the signs actually switch. It seems that neighborhoods with 11–20% poverty are 44% more likely than residents of neighborhoods with 0–10% poverty to have (or at least perceive that they have) indirect contact with public officials, while neighborhoods with high concentrations of poverty are 23% less likely to have even indirect ties to public officials. Possibly, this results from the fact that when there is less severe poverty in a neighborhood, there exists more of a sense that community problems are manageable, and strategies will be identified allowing community residents to contact public officials. On the other hand, neighborhoods with oppressive amounts of poverty may feel so overwhelmed that attempts to do anything or contact anyone about community problems seem futile. If Granovetter is correct, then the residents of these severely poor neighborhoods have an even more tenuous attachment to networks of political influence than appears at first glance. Consequently, we expect residents of these communities to have even less access to public officials and community leaders who could address the severe problems of their neighborhoods than other urban residents. As a result of such social isolation, we expect to find observable indicators of political isolation.

## PUBLIC OPINION AND NEIGHBORHOOD POVERTY

African Americans remain the most politically liberal group of citizens in the American polity (Dawson n.d.; Hamilton 1982; Kinder et al. 1989; Nie, Verba, and Petrocik 1979). However, we expect social isolation from the mainstream of the African American community to lead to political isolation not only on the part of the new black middle class but also on the part of those living in communities that have become isolated due to economic devastation.[17] We have seen that poverty-stricken neighborhoods were more likely to have a number of politically relevant characteristics of social isolation. Our concern here is whether devastated neighborhoods tend systematically to influence the political opinion of their African American residents differently from the rest of the African American community. In particular, we explore whether these neighborhoods inhibit feelings of political efficacy and community efficacy, whether they influence perceptions of other groups within the society, and whether they impinge on racial attitudes, as well as whether they influence the propensity of residents to participate in the political process. The question of participation will be addressed in some detail later. First, we shall consider whether poor neighborhoods condition the efficacy exhibited by residents, then briefly look at traditional measures of perceptions of the political process to see whether neighborhood poverty demonstrates an effect. From there, we evaluate the effect of neighborhood poverty on perceptions of different groups in society.

### Political Efficacy

If we begin with traditional measures of the political efficacy of participation it seems clear that severe neighborhood poverty, to varying degrees, has the ability to impact on perceptions of the efficacy of political acts, generally increasing a respondent's likelihood of viewing political actions as efficacious. The results listed in Table 2 indicate that living in a neighborhood with over 30% poverty increases the likelihood of a respondent's believing that it is effective to work for a candidate, attend a meeting, or go to see public officials or other people of influence with regard to community problems (see Appendix B). Only in the area of writing letters to public officials and others did neighborhood poverty of 21–30% demonstrate a negative effect.

These results are surprising because we might intuitively expect those in impoverished neighborhoods to indicate a lack of efficacy toward all political actions. However, we can hypothesize that those living in highly impoverished areas, with fewer resources, opportunities, and experiences upon which to base their evaluations of political actions believe that certain political acts unavailable to them are effective. Thus, individuals living in extremely poor neighborhoods who have never been asked to go to see a public official may, in fact, believe that such an act is effective. These respondents might further contend—accurately, in many cases—that individuals who do have access to resources and who do have opportunities to participate, publicly airing their grievances, actually do gain the attention and responsiveness of public officials. Thus, their evaluations of political efficacy may be in many cases not only an assessment of the act, in and of itself, but also a comment on the leverage to be gained from financial and social resources.

### Community Efficacy

When we turn our attention to questions of community efficacy, we find that neighborhood poverty again shows patterns of influencing how residents view their ability to alter community problems (see Appendix B). This is illustrated in Table 3.[18] First, the more impoverished one's neighborhood, the more likely one will agree that "community problems are so complicated that even informed people can't figure

TABLE 2

Neighborhood Poverty and Indicators of Political Efficacy

| | HOW EFFECTIVE TO | | | | |
|---|---|---|---|---|---|
| INDEPENDENT VARIABLE | WORK FOR POLIT. CAND. | ATTEND MEETINGS | GO SEE PUBL. OFFIC./ INFLUENT. PERS. | WRITE LETTERS | CONTRIBUTE MONEY |
| Log income | .07<br>(.02)*** | .01<br>(.03) | .02<br>(.03) | .01<br>(.11) | −.00<br>(.02) |
| Age | −.06<br>(.05) | −.01<br>(.06) | .03<br>(.06) | .06<br>(.07) | −.08<br>(.06)* |
| Education | −.13<br>(.06)*** | .00<br>(.07) | .04<br>(.08) | .12<br>(.08)* | −.03<br>(.06) |
| Indiv. poverty | .05<br>(.04) | −.03<br>(.06) | −.05<br>(.06) | −.03<br>(.06) | −.05<br>(.05) |
| Gender (woman) | .04<br>(.02)** | .04<br>(.03) | .00<br>(.03) | .03<br>(.03) | −.02<br>(.03) |
| Context. pov. 11–20% | .02<br>(.03) | .01<br>(.04) | −.00<br>(.04) | −.05<br>(.05) | −.02<br>(.04) |
| Context. pov. 21–30% | .01<br>(.03) | .04<br>(.03) | −.02<br>(.04) | −.08<br>(.04)** | .00<br>(.03) |
| Context. pov. 31+% | .06<br>(.04)* | .09<br>(.05)** | .10<br>(.05)** | .04<br>(.05) | .04<br>(.04) |
| Constant | .55<br>(.08)*** | .68<br>(.1)*** | .60<br>(.11)*** | .56<br>(.11)*** | .75<br>(.09)*** |
| N | 342 | 343 | 342 | 345 | 343 |

*Source*: Detroit Area Study 1989.
*Note*: Entries are unstandardized orindary least squares estimates followed by the standard error.
$^{***}p \leq .025$, one-tailed test.
$^{**}p \leq .05$, one-tailed test.
$^{*}p \leq .10$, one-tailed test.

them out." The more problems multiply due to the concentration of poverty within a neighborhood, the more pessimistic community citizens become regarding any hope that a solution can be found for their problems. The special nature of the most devastated neighborhoods in which African Americans reside—those with concentrations of poverty over 30%—is demonstrated in the responses to those questions that focus on the amount of one's influence to affect one's community. Only those respondents who live in the most concentrated areas of poverty showed any significant statistical difference from African Americans of less impoverished neighborhoods. Those in the very poorest neighborhoods were less likely to respond positively that they were satisfied with their level of community influence. Similarly, only those African Americans living in the poorest neighborhoods were less likely to believe that they had any direct influence over decisions of importance to their community.

Finally, two of our indicators of community efficacy—"hard to find good solutions" and "by working together people in community can influence decisions"—demonstrates a statistically significant positive effect at the poverty level of 11–20%. These findings in compilation begin to evidence a contrast in community efficacy between poor and more affluent African American communities. The indicators suggest that those in the poorest communities are more likely to doubt the ability of anyone to find solutions to community problems and more likely to doubt that they have any influence over what happens to their communities. However, people in less impoverished neighborhoods, while also finding community problems hard, may believe that by working together, they can influence decisions that affect their lives.

## Perceptions of Group Influence

African Americans generally have a strong, class-based view of the degree of influence which groups in society should have over "political and economic decisions" (see Appendix B). For example, 59% of African Americans believe that the working class has too little influence. Conversely, 76% believe that the upper class has too much influence; and 51% believe that businesses and corporations have too much influence. Not surprisingly, 74% believe that whites have too much influence.[19] However, devastated neighborhoods do have an effect on how African Americans view group influence. Table 4 displays the

**TABLE 3**

**Neighborhood Poverty and Perceptions of Community Problems**

| INDEPENDENT VARIABLE | EVEN INFORM. PEOP. CAN'T UNDERST. THE PROBLEMS | HARD TO FIND GOOD SOLUTIONS | I CAN INFL. DECISIONS | SATISFIED W/MY INFL. OVER DECISIONS | PEOP. WORKING TOGET. CAN INFL. DECISIONS |
|---|---|---|---|---|---|
| Log income | −.02<br>(.03) | −.02<br>(.03) | −.04<br>(.03) | .00<br>(.03) | −.03<br>(.02)* |
| Age | .14<br>(.07)*** | .24<br>(.08)*** | .04<br>(.06) | .17<br>(.07)*** | .12<br>(.05)*** |
| Education | −.01<br>(.08) | .06<br>(.09) | .11<br>(.08)* | −.05<br>(.08) | .14<br>(.06)*** |
| Indiv. Poverty | −.01<br>(.07) | −.03<br>(.07) | −.10<br>(.06)* | .01<br>(.06) | −.08<br>(.04)*** |
| Gender (woman) | .07<br>(.03)*** | .05<br>(.04) | .04<br>(.03) | .03<br>(.03) | −.03<br>(.02)* |
| Context. pov. 11–20% | .07<br>(.05)* | .12<br>(.05)*** | .06<br>(.05)* | −.01<br>(.05) | .05<br>(.03)* |
| Context. pov. 21–30% | .09<br>(.04)*** | .02<br>(.04) | .00<br>(.04) | −.00<br>(.04) | −.01<br>(.03) |
| Context. pov. 31+% | .13<br>(.06)*** | .08<br>(.06)* | −.10<br>(.05)** | −.22<br>(.05)*** | −.00<br>(.04) |
| Constant | .52<br>(.12)*** | .44<br>(.13)*** | .59<br>(.11)*** | .47<br>(.11)*** | .85<br>(.07)*** |
| N | 344 | 344 | 339 | 341 | 342 |

*Source*: Detroit Area Study 1989.
*Note*: Entries are unstandardized ordinary least squares estimates followed by the standard error.
***$p \le .025$, one-tailed test.
**$p \le .05$, one-tailed test.
*$p \le .10$, one-tailed test.

results from the multivariate analysis. The likelihood of support for working-class influence declines in neighborhoods that have tenuous ties to the labor market. Residents of neighborhoods with concentrations of poverty at the 20% level are modestly more likely than African Americans from less impoverished neighborhoods to believe that the working class has *too much* influence. However, residents of the most devastated neighborhoods were even more likely to believe the working class carried too much influence.

The distinction in evaluations of group influence among blacks in highly impoverished neighborhoods is also evident in their opinions on the influence of the middle class and labor unions. It should not come as a surprise that living in a very poor neighborhood increases the belief that the middle class has too much influence. Black citizens from the poorest neighborhoods were also more likely to believe that labor unions had too much influence. Among those whose communities have such unstable ties to employment, it should not come as a shock that they look with great suspicion on those classes and organizations that are perceived to be a part of, or benefiting from, the current order, even if the benefits are minimal as in the case of the working class. Thus, even the working class and their organizations receive less support from residents living in severe poverty than among other communities of African Americans.

Finally, it should be noted that in the very poorest neighborhoods, there is a modest indication that the residents in these neighborhoods are even more convinced that whites have too much economic and political influence than the already highly skeptical residents of other black communities. Two other indicators of perceived group influence—perceptions of influence of the upper class and influence of business and corporations—demonstrated no neighborhood effects among African Americans.

The social and economic isolation of African Americans in the poorest neighborhoods has led to a different pattern of beliefs about who has the correct degree of influence in American society. However, no such differences were evident when we examined the effects of neighborhood poverty on racial attitudes. Overall, we found no systematic differences in racial attitudes among our respondents. The opinions we examined ranged from "Generations of slavery and discrimination make it difficult for blacks" and "Blacks get less than they deserve" to questions of "preferential hiring for blacks." And while no systematic difference among neighborhoods could be identified, we believe that this finding should be qualified, because our previous research using data from the National Black Election Study found that

TABLE 4

**Neighborhood Poverty and Perceptions of Desired Group Influence**

| INDEPENDENT VARIABLE | WHO HAS TOO MUCH INFLUENCE? WORKING CLASS | MIDDLE CLASS | LABOR UNIONS | WHITE |
|---|---|---|---|---|
| Log income | −.01 | −.05 | −.02 | .05 |
| | (.03) | (.03)* | (.04) | (.03)** |
| Age | −.17 | .03 | −.05 | −.04 |
| | (.06)*** | (.07) | (.08) | (.06) |
| Education | −.21 | −.12 | .02 | −.04 |
| | (.08)*** | (.09)* | (.1) | (.08) |
| Indiv. poverty | .04 | −.09 | .01 | −.01 |
| | (.06) | (.07)* | (.07) | (.06) |
| Gender (woman) | −.01 | .01 | −.08 | .03 |
| | (.03) | (.03) | (.04)** | (.03) |
| Context. pov. 11–20% | .01 | .01 | .04 | −.03 |
| | (.04) | (.05) | (.05) | (.04) |
| Context. pov. 21–30% | .07 | −.01 | .03 | .03 |
| | (.04)** | (.04) | (.05) | (.04) |
| Context. pov. 31+% | .15 | .11 | .21 | .07 |
| | (.05)*** | (.05)** | (.06)*** | (.05)* |
| Constant | .34 | .51 | .56 | .72 |
| | (.1)*** | (.12)*** | (.13)*** | (.1)*** |
| N | 341 | 340 | 339 | 339 |

*Source*: Detroit Area Study 1989.
*Note*: Entries are unstandardized ordinary least squares estimates followed by the standard error.
$p \le .025$, one-tailed test.
$p \le .05$, one-tailed test.
$^{*}p \le .10$, one-tailed test.

individuals who are poor are much more likely to hold black nationalist beliefs. Thus, to the degree that these communities by definition contain concentrations of the poor, we should expect them to be relatively more receptive to ideas centered in a black nationalist ideology. Therefore, we might expect that questions measuring racial attitudes including concerns with black nationalism and independence might, in fact, demonstrate neighborhood effects.

We also examined a few other indicators of political attitudes and identification. Specifically, we focused on questions of party identification, liberal/conservative leaning, whether officials cared, and whether respondents considered officials incompetent. Again, overall there appeared no systematic relationship between any of these variables and neighborhood poverty. Only around the question of liberal/conservative identification did those in the poorest communities tend to be more liberal in their identification. Those in the poorest neighborhoods were statistically proven to be more liberal even allowing for the fact that African Americans as a group are extremely liberal. The only other minimal difference was the finding that residents of neighborhoods with 11–20% were more likely to believe that officials were incompetent.

We believe that the differences identified in public opinion by African Americans in poor neighborhoods could lead these residents to view potential coalition partners differently from African Americans from less devastated neighborhoods, thus possibly leading to even greater *political isolation*. With this understanding of neighborhood poverty effects on the attitudes of African Americans, we can now turn our attention to differences in the actual political behavior of respondents. To what degree do different living environments impinge on our opportunities, resources, and eventual choices with regard to participation in the political system?

## NEIGHBORHOOD POVERTY AND PARTICIPATION

We now take a closer look at the effect of African American neighborhood poverty on political participation. The question of participation among all African Americans has been critically connected to their quality of life. It is a well known tenet of African American history that the organized political activity of masses of the community from the 1950s through the early 1970s led to important gains in the area of civil rights, significantly altering the quality of life for many black people. Further, it is a well-learned lesson among most Americans that those who participate in politics are those who also gain a greater share of the pie. Congressional scholars have repeatedly confirmed such intuitions by demonstrating that the actions or votes of congressional members are in many instances tied to their perceptions of the wishes of the most vocal and politically active members of their districts (Fenno 1978).

In view of this evidence we suggest that if neighborhood poverty leads to the elimination or diminishing of participatory behavior by the African American poor, such a factor would be especially devastating; for without political activity among some of the United States' most marginal citizens, the African American poor, public officials would most likely demonstrate restricted accountability to the needs of those communities. Neglect of those already on the edge of survival would be especially devastating when deciding which issues belong on the public agenda, which programs are maintained or cut, and (generally) which communities deserve the attention and rewards of the government.

Our concern with the lack of participation on the part of the African American poor is especially relevant if, indeed, the poor exhibit different political opinions and policy prescriptions from the nonpoor. It has often been assumed by some scholars of democratic theory that while poor or disempowered Americans may not demonstrate patterns of political activity, there are citizens (often more affluent, with a higher class ranking and access to the government) who will pick up the torch of their ostracized sisters and brothers. In our case, African Americans with access to the political system are assumed to share the interests of those poorer members of the community missing from the political process. However, the data presented earlier suggest that we can no longer feel secure with the assumption that the African Americans able to make demands upon the political system hold similar views to those of all segments of the black community. This point is reinforced in one of the present authors' forthcoming work showing that poor African Americans are much more likely to support a black nationalist political program and redistributive policies than are other African Americans (Dawson n.d.). Thus, assumptions arguing that issues crucial to the survival of the poor (especially, the African American poor) will somehow find representation in the public arena come into question when we identify the lack of participation of the African American poor and differences in the public opinion of different members of the black community.

As stated earlier, both Wilson and Marable have argued that African Americans living in neighborhoods where there is extreme poverty may be expected to manifest political attitudes and behavior different from their "counterparts" living in less impoverished environments. We shall now utilize data gathered from African Americans living in the Detroit metropolitan area to test the impact of contextual poverty on participation. Does living in an impoverished neighborhood constrain the ability of individuals to participate actively and to influence the political decisions that influence much of the quality of their life?

Our analysis initially focused on five acts of generic participation:[20] Did the respondent vote for president in 1988? Did the respondent speak or write to some public official concerning an issue? Did the respondent attend a meeting about some important community problem? Did the respondent discuss politics with family or friends? and Did the respondent contribute money to a candidate, party, or organization?[21]

## A Model of Participation

We extended the model already presented to develop a general model of participation in local communities. As before, we are interested in testing the proposition that there exists a causal relationship between the level of poverty in a neighborhood and the political participation of its residents. Future research will focus on specifying the exact nature of such a relationship. In our model, we included the same individual level variables measuring socioeconomic status that we included previously. However, for this analysis we added one new variable known to affect political participation. The new independent variable used in the model of community participation was *mobilization*.

Mobilization has been a critical aspect of political participation in African American politics. At the local level we saw the massive mobilization for Harold Washington in Chicago's 1983 mayoral campaign. In both 1984 and 1988, massive mobilization was an integral component of both of Reverend Jesse Jackson's campaigns for president. Historically, political mobilization was critical in increasing black participation during both Reconstruction and the civil rights eras (Barker 1989; Foner 1988; Kleppner 1985; Morris 1984; Smith 1990). Mobilization is the activity designed to increase political involvement of citizens in a specific campaign. Mobilization among African Americans has occurred for voter registration drives, civil rights campaigns, and community organizing. Specifically, in this study, mobilization was measured positively if a respondent was asked to become involved in a specific form of political participation. A variable measuring mobilization (whether an individual was contacted to participate in the specific political act) was included where applicable.[22] The general model just described was then estimated with probit for each of the five dependent variables.

Before we detail the results from each equation demonstrating neighborhood effects, let us try to sketch out the general conclusions. Overall, the results indicate that the impact of neighborhood poverty on acts of participation seems to be limited and is generally constrained to the more severe levels of contextual poverty (over 20%). This is consistent with our earlier findings, as well as hypotheses that social isolation (and, in this case, political alienation and differences) are found in the more extreme categories

TABLE 5

**Neighborhood Poverty and Community Political Participation**

| INDEPENDENT VARIABLE | TALKS ABOUT POLIT. W/FAM./ FRIENDS | ATTENDED MEETING ABOUT COMMUN. ISS./PROB. | GAVE $ TO CAND./ PARTY/ GROUP |
|---|---|---|---|
| Log income | −.10<br>(.15) | −.13<br>(.16) | −.06<br>(.17) |
| Age | .57<br>(.33)** | .62<br>(.36)** | .48<br>(.4) |
| Education | 1.20<br>(.42)*** | .90<br>(.41)*** | 1.07<br>(.44)*** |
| Indiv. poverty | −.31<br>(.3) | −.59<br>(.32)** | −.32<br>(.34) |
| Gender (woman) | .10<br>(.15) | .06<br>(.17) | .09<br>(.18) |
| Mobilized (contacted) | — | 1.27<br>(.16)*** | 1.29<br>(.19)*** |
| Context. pov. 11–20% | .04<br>(.24) | .18<br>(.23) | −.09<br>(.23) |
| Context. pov. 21–30% | −.42<br>(.19)*** | .31<br>(.20)* | −.25<br>(.21) |
| Context. pov. 31+% | −.51<br>(.24)*** | −.51<br>(.33)* | −.72<br>(.34)*** |
| Constant | 5.50<br>(.54)*** | 3.75<br>(.57)*** | 3.52<br>(.62)*** |
| N | 345 | 345 | 343 |

*Source*: Detroit Area Study 1989.
*Note*: Entries are probit estimated followed by the standard error.
$^{***}p \leq .025$, one-tailed test.
$^{**}p \leq .05$, one-tailed test.
$^{*}p \leq .10$, one-tailed test.

of contextual poverty. Our data indicate that the effects of contextual poverty on political participation are evidenced when the threshold of severe neighborhood poverty (over 21%) has been crossed. Table 5 lists our specific findings on the impact of neighborhood poverty on participation.

The acts upon which contextual poverty demonstrates the most consistent effect are (1) giving money to a candidate, group, or party; (2) talking to family or friends about politics; and (3) attending a meeting about a community problem (see Appendix B). At the level of 31% and above, significant negative effects were estimated on the dependent variable, "Have you contributed money?" Respondents were 14% less likely to give money than those in less devastated neighborhoods. Again, these effects occurred even after we controlled for individual household poverty. The impoverished character of the neighborhood directly contributed to a lower likelihood of contributing money. While this finding may not seem that surprising, it should be understood in the context that African Americans do have a history of contributing to African American candidates and social institutions even if those outside the community perceive the amounts of contribution to be minimal. We can hypothesize that neighborhood poverty may *inhibit the number of opportunities or requests* an individual receives to contribute money. Solicitors probably will not go into certain neighborhoods viewed as a waste of time or as "dangerous." Further, living in an impoverished neighborhood may alter one's perception of the efficacy or salience of giving money to a candidate, group, or party. Therefore, a destitute living environment may directly alter one's political and economic priorities. Whatever the reason for neighborhood poverty's impact on acts of giving money, the data clearly allow us to conclude that in fact, there is a relationship between giving money and the level of neighborhood poverty in which one lives.

A similar negative relationship exists between neighborhood poverty and the probability of talking to one's family about politics. It seems that individuals living in neighborhoods with severe poverty (at least 21%) are less likely to talk with family or friends about politics. Those who live in neighborhoods with at least 21% poverty were 13% less likely, while those who lived in the poorest neighborhoods were 17% less likely to talk to family and friends about politics, compared to African Americans living in less impoverished communities. A possible explanation may hinge on the lack of formal political information provided to those living in neighborhoods with severe contextual poverty. Also, families in such neighborhoods may have what they perceive to be more immediate and important issues dominating their discussions. Although we cannot say conclusively without further analysis what the reason is for this finding, our data suggest that neighborhood poverty negatively impinges on the probability that a respondent living in a neighborhood with at least 21% poverty will talk to family and friends about politics.

A final activity under examination concerned the relationship between attending a meeting and neighborhood poverty. The results just detailed, when combined with the 12% negative effect of extreme neighborhood poverty on the probability of attending a meeting, further confirm our suspicions that even after controling for other factors, extreme neighborhood poverty acts to weaken the attachment of residents to the participatory process of "democratic" life. It is interesting to note, however, that 21–30% contextual poverty demonstrated a positive effect, increasing by 8% the probability that residents would attend a meeting. We can only hypothesize that such a finding is related to differences in the perceived and actual manageability of problems, as well as differences between communities in the opportunities to attend meetings.[23]

Recognizing the impact of neighborhood poverty on participation, it therefore becomes the urgent task of researchers to (1) identify those factors that have worked to inhibit the political participation of poor members of the African American community; (2) explore the mechanisms that can alleviate or circumvent barriers to participation, increasing the ability of poor members of our community to participate in local, regional, and national decisions; and (3) expand our understanding of the effects of poverty on political behavior to include not only individual measures of poverty but also the environmental or contextual consequences of living in impoverished neighborhoods.

## CONCLUSION

The preliminary analysis presented here suggests that there are, indeed, significant differences between persistently poor and other neighborhoods. Throughout our analysis, we found evidence that African Americans living in the most impoverished neighborhoods held different political attitudes and exhibited different political behavior, apart from the impact of individual poverty. First, our indicators suggest that the immediate social isolation that researchers have identified in poor neighborhoods can, in fact, be measured. Political and economic networks developed through group membership are often unavailable to African Americans living in extremely poor neighborhoods. Thus, social contacts that can sometimes provide political access are inhibited in poor neighborhoods. Further, we know that there exists an identifiable impact of neighborhood poverty on perceptions of the effectiveness of political acts, perceptions of community efficacy, and perceptions of group influence, with residents of the poorest neighborhoods demonstrating the most consistent effect. Ironically, the results somewhat suggest that African Americans who live in the poorest neighborhoods are more likely to believe in the efficacy of some types of political action even though they are less likely to engage in such actions. Even in the area of political participation, neighborhood poverty effects were evident. However, mobilization, when applicable, seems to be one effective way to increase the probability of individuals' engaging in political actions. Again, it is important to remember that although our measures of neighborhood poverty were mere approximations from census tracts, even with such a crude measure, effects were identifiable beyond the impact of individual poverty.

The findings also offer evidence that the effects of social isolation and economic distress on the poor may be leading to the lack of confidence in black group effectiveness predicted by Marable and to the continued class divisions predicted by Wilson, various pluralists, and many Marxists. However, these class divisions are not detected very well by traditional measures of partisanship and political choice presented within the two-party system or mainstream politics. They are most noticeable in areas related to evaluations of groups in the polity, politically relevant measures of social isolation, and beliefs about how politically efficacious one is in relation to one's community. Behaviorally, African Americans who live in poor neighborhoods are less likely to participate than other African Americans. This lack of engagement of the African American poor should cause concern for all those who care about the working of democracy, even those who cherish stability in a democracy and welcome such inactivity on the part of the poor.[24] Przeworski (1975) suggests that those with strong political views who withdraw from the political arena, rather than those who enter it, are often the source of political instability.[24] Indeed, those who are most radical and least engaged are clearly those with the least stake in a given political system.

Most black people, regardless of poverty status, agreed on the evaluation of the president, the need for a strong state, redistributive economic policies, and choices for elected offices. It is not surprising that when these measures are evaluated, evidence for political differences based on economic status are sometimes hard to find. Differences and divisions within the black community often occur outside of, and/or orthogonal to, the debates of mainstream political America. In this context, the lack of political engagement, efficacy, and participation by the urban black poor take on a different light. Scholars should consider that the political disaffection of the black urban poor may be due, in part, to the lack of political leaders and organizations representing their interests in the mainstream of American politics.

The rediscovery of Malcolm X in many hard-core urban neighborhoods can be reinterpreted in the light of these and other recent findings. Individually, poor African Americans are more supportive of black nationalist ideas. When combined with the perceived lack of solutions to neighborhood problems by residents of devastated neighborhoods and their dissatisfaction with their influence to get the "system" to respond, we should not be surprised that they turn to a black nationalist philosopher who worked outside of the system and challenged the very notion of the legitimacy of the state. Malcolm X most clearly argued that poor African Americans had few obligations to the American state, since it failed to be minimally responsive to the needs of poor African Americans.

African American criticism of the justness of American democracy neither began or ended with Malcolm X. In the last years of his life, Dr. Martin Luther King argued that America was an unjust, profoundly racist, and exploitative society. He argued, "There is a fire raging now for the Negroes and the poor of society. They are living in tragic conditions because of the terrible economic injustices that keep them locked in as an 'underclass' as the sociologists are now calling it" (M. King 1986, 650). He further stated that the poor live in a "cruelly unjust society" and that a nonviolent revolution was needed to bring about a just and democratic society (p. 647). King would also not have been surprised that the poorest African Americans were alienated from affluent African

Americans. He lamented that many "middle-class Negroes" had developed a "selfish detachment" and had forgotten that their status was directly due to the struggles of less affluent African Americans for justice and equality. Du Bois (1986) also rejected the concept that African Americans lived in a democratic system. He argued that a lack of democracy extended both to poor whites within America and to people of color throughout the world. The hostility of the American state toward African Americans and its inability to provide either physical or economic security is a constant theme of black political theory, literature, and letters.

In the modern era, two factors are significant for understanding what is different about the feelings of detachment and exclusion from American democracy on the part of African Americans living in poor neighborhoods. One is the growing economic polarization *within* the African American community (alluded to by King), such that there is some evidence of growing political divisions within the African American community based on class (Cohen n.d.; Dawson n.d.). Again, we put forth this argument not to suggest that race is no longer a primary factor in determining the quality of life for African Americans. Instead, we suggest that class, in a "postmodern society," where economic opportunities have been strategically dispersed, takes on an increasingly important role in understanding the divisions of African American political (in)action. A second factor is the attenuation of life chances as they become even more firmly wedded to the *place* where one resides. Persistently poor neighborhoods provide little opportunity for self-advancement and mobilization. Young argues that social justice is based on the twin realities of (1) being able to develop oneself and express one's own experiences and (2) the ability to participate in determining "one's action and the conditions of one's actions" (Young 1990). Neither condition exists for African Americans who live in the most devastated neighborhoods. As a result our research suggests that residents of these neighborhoods have become politically alienated. The intensified breakdown of democracy and justice in these neighborhoods should cause concern in society at large for instrumental (if no other) reasons.

We would be remiss if we did not return to the words of philosophers. We begin with Locke's concern about the stability of state systems. He remarks, "There is only one thing which gathers people into seditious commotions, and that is oppression" (Locke 1955, 54). Du Bois, concerned with injustice and equality contends, "To be a poor man is hard, but to be a poor race in a land of dollars is the very bottom of hardships (Du Bois 1961, 20). In a world in which the age-old problems of economic oppression are too easily forgotten, political disruption may increasingly be the only way to challenge such oppression. Unfortunately, the riots of Newark and Detroit in 1967 and Los Angeles in 1992 prove this prognosis all too true.

## APPENDIX A

The control variables of individual-level socioeconomic status and mobilization and their codings are as follows (Detroit Area Study 1989):

*Family Income.* The log of family income (Linc). Income was coded as a 24-category variable ranging from less than $3,000 to $90,000+.

*Age.* Coded as an 8-category variable:
0 = 0–19
.14 = 20–29
.29 = 30–39
.43 = 40–49
.57 = 50–59
.71 = 60–69
.86 = 70–79
1.0 = ≥ 80+.

*Education.* Coded as a 5-category variable:
0 = completed less than 9th grade
.2 = 9–12 years
.4 = high school diploma
.6 = 1–3 years of college
.8 = bachelor's degree
1.0 = graduate degree.

*Gender.* 0 = male, 1 = female.

*Individual Poverty.* Binary (dummy) variable: 1 = $10,000 per year; 0 = ≥ $10,000 per year.

*Mobilization (only on questions of participation).* Binary (dummy variable: 1 = contacted and asked to engage in specific forms of participation; 0 = no contact or mobilization.

## APPENDIX B

The dependent variables of social isolation, political efficacy, community problems, group influence, and political participation and their codings are as follows:

*Social Isolation*
Member of a church organization
  dichotomous: 1 = member
Belongs to a group
  dichotomous: 1 = belongs to group
Knows someone who knows someone influential
  dichotomous: 1 = knows someone
Victim of a crime
  dichotomous: 1 = has been a victim of crime
Thinks crime is a serious problem in neighborhood
  0 = not a problem, .5 = somewhat a problem, 1 = very serious problem

*Political Efficacy*
0 = not at all effective, .33 = not very effective, .67 = somewhat effective, 1.0 = very effective

*Community Problems*
0 = disagree strongly, .33 = disagree, .67 = agree somewhat, 1 = agree strongly

*Group Influence*
0 = too little influence, .5 = just about right, 1 = too much influence

*Political Participation*
Talks about politics with family or friends
  dichotomous: 1 = discussed politics
Has attended a meeting about community issue or problem
  dichotomous: 1 = attended a meeting
Gave money to a candidate, party, or group
  dichotomous: 1 = gave money

## APPENDIX C

The contextual poverty variables and the relative frequency of each census tract in each category are as follows (Detroit Area Study 1989):

*Context. Pov. 0–10%*

| *Tract* | *Frequency (%)* |
|---|---|
| 1014 | 2.8 |
| 1033 | .7 |
| 1036 | .7 |
| 5003 | 1.4 |
| 5015 | .7 |
| 5051 | 2.1 |
| 5061 | 7.7 |
| 5305 | 10.5 |
| 5347 | 7.7 |
| 5386 | 7.0 |
| 5389 | 7.0 |
| 5394 | 8.4 |
| 5409 | 1.4 |
| 5415 | 7.0 |
| 5421 | 11.2 |
| 5428 | 3.5 |
| 5433 | 2.8 |
| 5441 | 5.6 |
| 5467 | 11.2 |
| 5701 | .7 |

*Context. Pov. 11–20%*

| *Tract* | *Frequency (%)* |
|---|---|
| 1088 | 3.8 |
| 5004 | 1.3 |
| 5038 | 12.6 |
| 5073 | 8.8 |
| 5103 | 11.4 |
| 5133 | 12.6 |
| 5248 | 17.7 |
| 5369 | 12.6 |
| 5388 | 11.4 |
| 5526 | 1.3 |
| 5710 | 6.3 |

*Context. Pov. 21–30%*

| *Tract* | *Frequency (%)* |
|---|---|
| 5076 | 2.2 |
| 5143 | 8.7 |
| 5175 | 12.3 |
| 5180 | 15.2 |
| 5254 | 7.2 |
| 5308 | 8.0 |
| 5323 | 15.2 |
| 5336 | 11.0 |
| 5370 | 12.3 |
| 5453 | 8.0 |

*Context. Pov. 31+%*

| *Tract* | *Frequency (%)* |
|---|---|
| 5078 | 3.0 |
| 5129 | 10.6 |
| 5151 | 16.7 |
| 5216 | 6.0 |
| 5223 | 19.7 |
| 5321 | 15.2 |
| 5343 | 13.6 |
| 5345 | 15.2 |

Contextual poverty of 0–10% involves 20 tracts, 143 cases, and 31.9% of the sample; 11–20% involves 11 tracts, 79 cases, and 17.6% of the sample; 21–30% involves 10 tracts, 138 cases, and 30.8% of the sample; 31+% involves 8 tracts, 66 cases, and 14.7% of the sample. Twenty-two cases did not have contextual information.

## APPENDIX D

Table D-1 shows converted probabilities for the probit coefficients. Included in this table are the populaton estimates of the "average effect" of contextual poverty on dichotomous dependent variables. The estimates were generated through calculating the estimated effect for each case and summing the individual estimates.

**TABLE D-1**

**Conversion to Percentages of Statistically Significant Coefficients Generated Using Probit**

| DEPENDENT VARIABLES, USING PROBIT FOR ANALYSIS | CONTEXTUAL POVERTY 11–20% | 21–30% | 31%+ |
|---|---|---|---|
| Member in a church organization | — | — | −24*** |
| Belongs to a group | — | — | −20*** |
| Victim of a crime | 9* | 8* | 13* |
| Knows someone who knows someone influential | 44*** | — | −23*** |
| Talks about politics with family or friends | — | −13*** | −17*** |
| Has attended a meeting about community issue or problem | — | 8* | −12* |
| Gave money to a candidate, party, or group | — | — | −14*** |

*Source*: Detroit Area Study 1989.
$^{***}p \le .025$, one-tailed test.
$^{**}p \le .05$, one-tailed test.
$^{*}p \le .10$, one-tailed test.

## Notes

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1. A particularly trenchant, if problematic, statement of this thesis can be found in Jencks 1988.

2. For an exception, see Berry, Portney, and Thomson 1991. The term *deadly neighborhoods* comes from Jencks's (1988) article by that name. However, we are concerned with a quite different problematic from the one Jencks speculates about.

3. Part of the problem when discussing "the" poverty literature is that there exists to a significant degree two communities of discourse. See Dawson and Wilson 1991 for a discussion on how scholarship within political science is shaped by race.

4. Adults 18 years or over, who resided in Wayne, Oakland, and Macomb counties were eligible for inclusion in the sample. The response rate was 70.2%.

5. In this version of our work, *neighborhood* is defined as "census tract." Although we realize that many have argued that census tracts are not neighborhoods, it is currently our best indicator of neighborhood conditions. In future research, we hope to map census tracts more closely to self-defined neighborhoods.

6. Indeed, Berry and his colleagues find extremely few excessively poor neighborhoods within the cities included in their study (Berry, Portney, and Thomson 1991).

7. We prefer to use the term *persistently poor neighborhood,* instead of *underclass neighborhood.* For comments on the misuse of the term *underclass,* see Gans 1990, 271–77; Reed 1991.

8. We utilize Detroit to illustrate issues concerning the spatial concentration of poverty because of the constraints of the Detroit Area Study. However, Detroit is representative of other urban areas confronting such poverty. For a further discussion of the comparability of Detroit to the United States, see Detroit Area Study 1989.

9. The description of contextual measures and their distribution used in this analysis can be found in Appendix C. The general distribution of cases in our four categories is as follows: neighborhood poverty in 0–10% range = 37% of the sample; poverty in the 11–20% range = 18%; poverty in the

21–30% range = 32%; and poverty in the 31%-and-above range = 13%. The total of African American cases in our study is 448.

10. Some analyses were performed using a category of 40% contextual poverty and above. Despite some interesting findings, we refrain from reporting the results directly in the text because of the small number of cases upon which the analysis was performed.

11. See one of the present author's *Behind the Mule* (Dawson n.d.) for a more comprehensive treatment of African American public opinion and political behavior.

12. Gary King (1991), in an article exploring questions of model specification, suggests that political scientists spend too much time attempting to specify the "true" model when they should pay more attention to identifying and understanding specific causal relationships.

13. The independent variables were coded in numerous ways for this analysis. Age was grouped into seven cohort categories. Individual poverty status was dichotomized between poor and nonpoor on the basis of reported income. Income was logged and education was categorized into eight levels.

14. One of the present authors also establishes that African American beliefs are substantially different from those of white Americans (Dawson n.d.).

15. Granovetter uses the term *weak ties*. In order to avoid confusion with "weak" political and labor force attachment, we refer, instead, to "secondary," or "loose," ties.

16. The probabilities for the probit coefficients are the population estimates of the "average effect" of contextual poverty on a dichotomous dependent variable. The estimates were generated by calculating the estimated effect for each case and summing the individual estimates. See Table D-1 for full list of converted probabilities.

17. See Landry 1987 and Boston 1988 for descriptions of the socioeconomic characteristics of the new black middle class. Boston also provides a short discussion of the political implications of the changes in the black class structure.

18. Individual African Americans exhibit the same degree of personal efficacy regardless of the degree of poverty within their neighborhoods.

19. The patterns displayed by black Detroiters are similar to national patterns evidenced by African Americans. For one example of black responses to a slightly different question that asks only about political influence, see Tate and her colleagues' (1988) summary of the 1984 National Black Election Study.

20. "Generic" in this context means acts of participation that respondents were asked about not directly tied to community issues such as education and crime.

21. We wanted to include analysis of nontraditional forms of participation, such as participation in a boycott or protest. However, the numbers of respondents acknowledging participation in such acts was too small to allow for reliable statistical analysis.

22. Mobilization indicators, included in this analysis, are coded as dichotomous variables.

23. It is important, however, to note that neighborhood poverty did not have an effect on whether an individual chose to vote or had contacted an official—both individual forms of participating centered on government responsiveness.

24. We also began to see signs of a secondary neighborhood poverty effect in communities with lower levels of poverty. In communities where problems can be perceived as manageable and where the work of residents sometimes results in victories, this type of activity may lead to sense of empowerment. Only further analysis can really explore this question.

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# CITIZEN ACTIVITY: WHO PARTICIPATES? WHAT DO THEY SAY?

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*We use responses to a large-scale national survey designed to oversample political activists to investigate the extent to which participant publics are representative of the public as a whole. Building upon the finding that while voters differ from nonvoters in their demographic attributes, their attitudes as measured by responses to survey questions are not distinctive, we consider a variety of political acts beyond voting that citizens can use to multiply their political input and to communicate more precise messages to policymakers. In addition, we consider not only respondents' demographic characteristics and policy attitudes but also their circumstances of economic deprivation and dependence upon government programs. Although activists are representative of the public at large in terms of their attitudes, they differ substantially in their demographic attributes, economic needs, and the government benefits they receive. Furthermore, in terms of the issues that animate participation, groups differentiated along these lines bring very different policy concerns to their activity.*

Questions of representation, long central to democratic theory, present themselves in several contexts in empirical studies of American politics. They constitute one of the fundamental theoretical anchors in studies of legislatures and other deliberative bodies; they arise in considerations of what kinds of organizations—and what kinds of interests—are active in pressure politics; and they are germane to understanding citizen political participation. Through the multiple forms of political activity, citizens have an opportunity to communicate their concerns and wishes to political leaders and thus to attempt to influence public outcomes. Those in public life are more likely to be aware of, and to pay attention to, the needs and preferences of those who are active.[1] Thus, it would seem to matter for the democratic principle of equality that studies of citizen participation in America concur in showing political activists to be unrepresentative in their demographic characteristics of the public at large.[2]

That political activists are not in demographic terms descriptively representative of the public at large does not necessarily imply, however, that there is a gap in policy preferences between those who take part and those who do not. Wolfinger and Rosenstone apply to voter turnout an important lesson from the literature on elected assemblies, namely, that although elected representatives are more likely to be male, well-educated, affluent, and drawn from the dominant racial and ethnic groups, demographic characteristics are a very imperfect predictor of either their policy preferences or their behavior in office (1980, 108–14).[3] Wolfinger and Rosenstone confirm the demographic differences between voters and nonvoters, but they also demonstrate that differences between the public and the voters on such issues as government welfare policy, health care, and abortion are negligible. (The exception is party identification, where there is a genuine, though not terribly large, Republican electoral advantage among voters in comparison with the public as a whole.) Moreover, any small differences between voters and nonvoters in attitudes on these policy issues are not systematically in a liberal or conservative direction. On issues of domestic economic policy, other researchers find some tilt in a liberal direction among nonvoters but agree that the differences are not very great.[4]

We use data from a large-scale study of citizen activity that was designed to include a disproportionate number of political activists in order to explore the question of the representativeness of the participant population. (See Appendix A for a description of the sample.) Our data on voting and policy preferences are consistent with Wolfinger and Rosenstone's. Using responses to standard seven-point-scale survey questions, we replicated Wolfinger and Rosenstone's analysis of the representativeness of the attitudes of voters with respect to such issues as whether the government should provide all citizens with an adequate job and standard of living or let each individual get ahead on his or her own and whether the government should reduce social spending or maintain government services. In terms of the question about government provision of jobs, voters are slightly more conservative than the public as a whole. Otherwise the differences are minuscule. In short, our data support the conclusion that voters and nonvoters do not seem to differ substantially in their attitudes on public policy issues.

For several reasons, however, data about policy preferences of voters and nonvoters construe much too narrowly the problem of the representativeness of the participatory input. First, the lessons contained in the literature on representation in pressure politics (in which the existence of politically relevant, jointly interested constituencies is taken as axiomatic and potential constituencies not represented by organizations are thought to be at a disadvantage in the policy process)[5] suggest that we should not simply dismiss demographic characteristics as irrelevant even when they are not associated with differences in opinion on public issues. When demographic distinctions are pertinent to political conflict (as they often are), disparities in participation may be significant. There is implicit information contained in the conspicuous social characteristics of activists, particularly when those characteristics are relevant to public policy. When a group (especially one with identifiable, politically relevant attributes) is active, it becomes visible to an elected representative and is incorporated into his or her salient constituency. Although they do not necessarily have an accurate reading of the overall distribution of opinion within the constituency on each policy issue, politicians attend to their constituencies and know who is watching what they do. (As a former representative is said to have remarked about the town in which two of us live, "One-tenth the votes. Half the mail.") Even in the absence of explicit directives (and constituents often do not send detailed messages), elected officials anticipate the needs and make inferences about the preferences of potentially active constituents.[6] Thus, it matters not only how participants differ from the nonparticipants in their opinions (whether they want higher or lower taxes, greater or lesser attention to social welfare) but who they are.

Comparisons between voters and nonvoters in terms of their policy attitudes cannot fully address the issue of the representativeness of activist publics in that they focus only on differences in preferences as revealed in questions about public issues preselected by authors of surveys. These policy issues are not necessarily the matters of most concern to respondents. Even more important, they are not necessarily the issues that activists address when they actually take part in politics.

In addition, voting is only one of the many kinds of participation; and it differs from other forms of participation in several ways that are germane to our concerns. For one thing, the vote is the one participatory act for which there is mandated equality: each citizen gets one and only one. Other forms of activity necessitate no such equality of inputs. Individuals may make as many phone calls to public officials, spend as many hours campaigning for as many candidates, and attend as many demonstrations as their time and inclination permit. Within certain limits, they can even write as many checks of whatever size their bank balances allow. Moreover, for some forms of participation, when the volume of activity is multiplied, the possibility that it will be accompanied by pressure to respond is enhanced. A candidate can ignore with impunity a single voter or a single letter writer, even one who writes frequently. The campaign volunteer who works many hours and the donor who makes a large contribution has potentially greater leverage. Finally, the vote also differs from many kinds of activity in being a rather blunt instrument for the communication of information about the needs and preferences of citizens. In contrast to the vote, a letter to a Senator, a sign carried at a protest, and a conversation between members of a neighborhood group and a city councillor all permit the transmission of much more precise messages about citizen concerns.

We examine the representativeness of participatory input by considering participation beyond the vote and by considering characteristics other than positions taken on standardized policy scales. In particular, we shall distinguish between policy attitudes as revealed by answers to survey questions about policies and policy-relevant circumstances, that is, various social and economic characteristics that make government policies relevant to an individual. We focus on respondents' economic attitudes and economic situation, considering not only their incomes, education, and attitudes on issues of economic and welfare policy but also the extent to which they have faced real economic deprivation in recent times and are dependent upon various governmental programs. Furthermore, we consider a variety of political activities in addition to voting: working in electoral campaigns, making campaign contributions, getting in touch with public officials, attending protests or demonstrations, engaging in informal efforts to solve community problems, attending meetings of a local governing board or council on a regular basis, and serving in a voluntary capacity on such a board. In addition, we examine the *amount* of political activity—not only the number of different activities but also the volume for any particular act (e.g., the number of dollars donated and the hours devoted to campaigning or to contacts with public officials). Finally, we investigate the actual content of the participatory input probing the nature of the policy concerns that animate political activity and the messages that accompany participatory acts.

## POLICY RELEVANT CIRCUMSTANCES AND POLICY ATTITUDES: WHO YOU ARE, WHAT YOU WANT, AND WHAT YOU NEED

We begin by considering the level of political activity of population groups defined in terms of who they are, what they want, and what they need. Figure 1 considers several characteristics connected to economic position and political preferences in order to determine in what ways activists differ most from the population as a whole: attitudes expressed on standard survey questions, demographic characteristics, or economic circumstances and needs.

Our consideration of economic needs and circumstances concentrates on two dimensions: efforts by respondents (and others in their households) to get by economically and receipt of various government benefits by respondents or immediate members of their families living with them. In our survey we asked whether, in order to make ends meet, the respondent or any immediate family member living in the household had to "put off medical or dental treatment," "cut back on the amount or quality of food," or "delay paying the rent or making house payments." We inquired, as well, about two other strategies for making ends meet: did anybody "cut back on spending on entertainment or recreation"? and did anybody "work extra hours or take an extra job"? In addition, we asked whether the respondent or a member of the immediate family living in the household received means-tested government benefits (food stamps, subsidized housing, Medicaid, or Aid to Families with Dependent Children [AFDC]) or non-means-tested benefits (Social Security, veterans' benefits, Medicare, or educational loans).[7] We are thus able to locate those who have real financial needs or depend upon government programs. These measures are, of course, closely related to income; but they give a more direct indication of respondents' potential interests with respect to government support.

Figure 1 uses an overall additive measure of political activity, based on all the political acts about which we asked in our survey, thus expanding our view of participation beyond the vote.[8] The score for any group is simply the mean number of political acts. (Exact wording for these and other questions is in Appendix C.) The data make clear that large differences in attitudes on public issues or partisan attachments are associated with little variation in political activity compared with the substantial differences in activity of groups distinguished by their significant demographic characteristics, their needs, or their receipt of government benefits. Note that self-identified liberals and self-identified conservatives are slightly more active than the population as a whole, with ideological moderates somewhat below average in activity. Similarly, self-identified Republicans and Democrats are somewhat, though not substantially, more active than the population at large.

We are concerned not only about differences in policy preferences as measured by responses to survey questions (which questions tell us neither whether respondents care about an issue nor whether they might be active in relation to it) but also about differences in who people are and their actual life circumstances.[9] With respect to socio-economic status (income and education), the activity disparities among groups are much more substantial. For example, those with family incomes over fifty thousand dollars per year score much higher in overall participation than those with incomes under twenty thousand dollars. These demographic differences are paralleled by substantial differences in rate of political activity across groups distinguished by financial need or by the receipt of services. Consider the activity of those who report some financial pinch. They are less active than the population as a whole, with the divergence from the average for all citizens increasing with the severity of the financial squeeze. Those who report the relatively mild—and quite widespread—recourse of cutting back on recreation do not differ from the population as a whole very much. Those who had to cut back on spending for food or who delayed paying the rent, however, are substantially less active. Clearly, those with real needs are less visible.

This pattern is even more pronounced if we consider those who reported that they or a member of the immediate family received one of a number of government benefits. The receipt of benefits per se does not imply a low level of activity. Those who receive non-means-tested benefits such as student loans or veterans' benefits, Medicare, or Social Security are at least as active as the public as a whole. In contrast, those who receive means-tested benefits such as AFDC, Medicaid, food stamps, or subsidized housing are substantially less active than is the public as a whole. The differences imply that those who would be in most need of government response because they are dependent on government programs are the least likely to make themselves visible to the government through their activity.

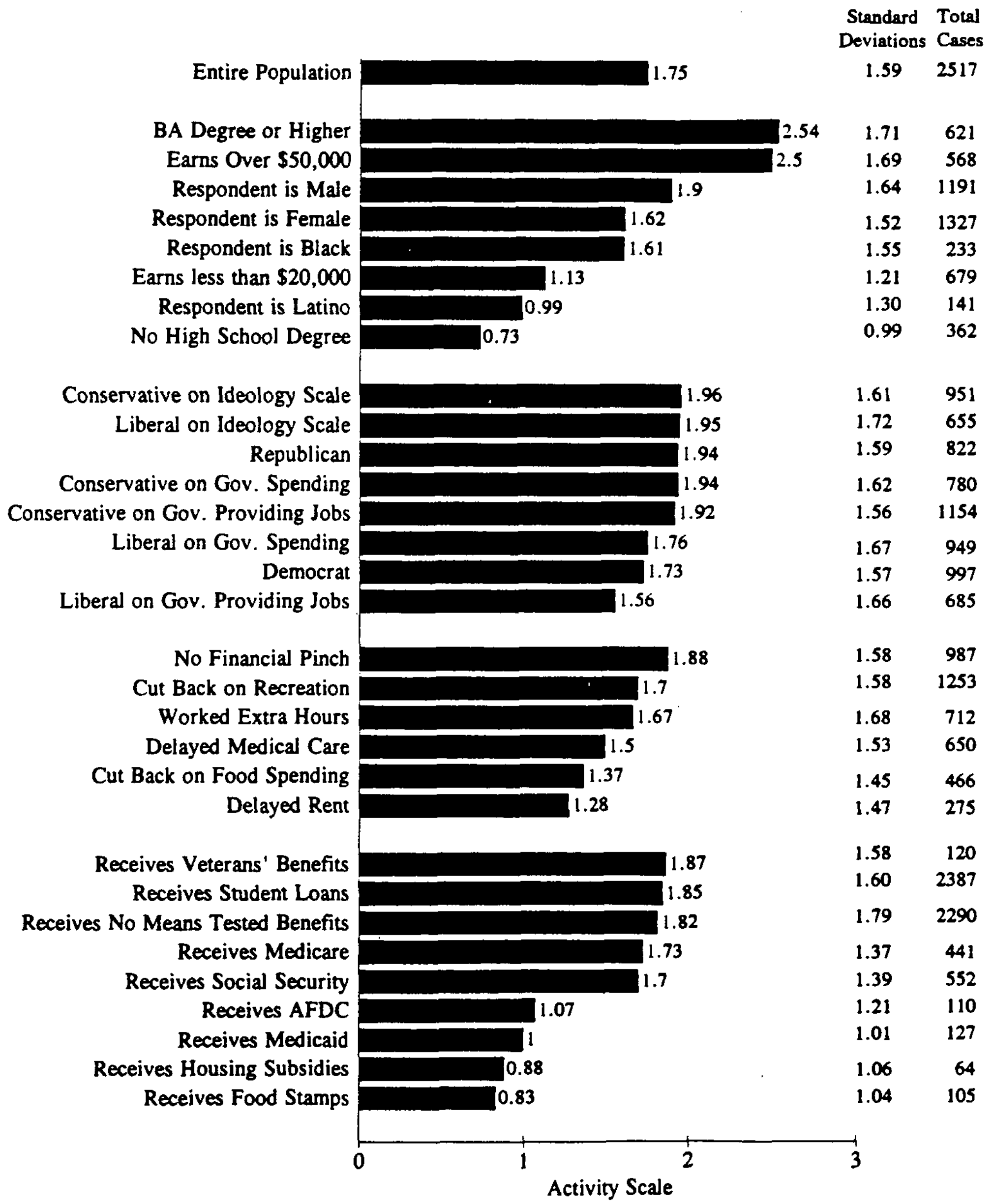
## What You Want, Need, and Get—and What You Do

It is useful to decompose the data in Figure 1, in which activity is measured by a summary of several participatory acts, in order to consider the kinds of participation separately. As we have seen, voters do not differ much from nonvoters when it comes to policy preferences. But what about in terms of their actual needs or their actual dependence on government programs? And do those who do more than just vote—who engage in participatory acts that may involve more pressure or convey more information—differ from the population as a whole?

Since it is difficult to compare representation across different activities and different characteristics, we use a summary index of representation based on a generalization of the index developed by Wolfinger and Rosenstone (1980, 105–14). Our measure of over- and underrepresentation is based on the ratio of the percentage that a particular group is of the activist segment of the population to the percentage that group is of the population as a whole. By taking the log of the ratio, we create a logged representation scale (LRS) with some useful properties. Because the LRS is a dimensionless number like a correlation coefficient or beta weight, one value can be compared with another. Furthermore, it is a symmetrical index: it ranges from plus infinity to minus infinity. An index of zero indicates that a group is represented exactly proportionately; negative numbers, that it is underrepresented; positive numbers, that it is overrepresented. The scores on our LRS scale are the logarithm of the ratio of the likelihood that a group member is in the activist population to the likelihood that any individual is in the activist population. An

FIGURE 1

**Mean Activity Score by Demographic Characteristics, Attitudes, Needs, and Receipt of Benefits**

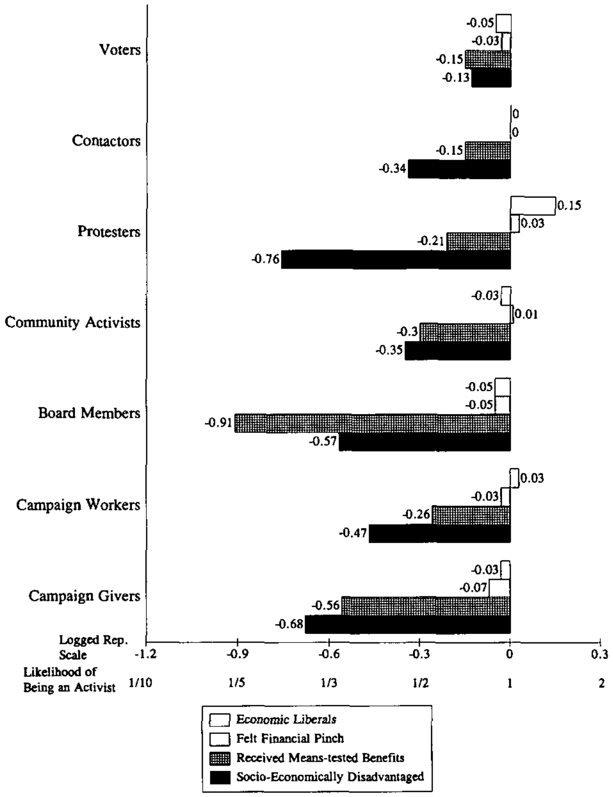


LRS score of .30 indicates that a group member is twice as likely to be in the activist portion of our sample than in the population as a whole; an index of −.30 indicates that the group member is half as likely to be found among the activists. An LRS score of 1 indicates overrepresentation by a factor of 10; −1 indicates underrepresentation by the same amount. To aid in understanding our measure, we put these ratios that indicate the degree of over- and underrepresentation—labeled "likelihood of being an activist"—below the LRS scores on Figure 2 and subsequent figures. (A more complete explanation of this measure is contained in Appendix B.)

Figure 2 reports for various kinds of activity the LRS for groups of individuals defined by their political attitudes on economic issues;[10] circumstance of socio-economic disadvantage;[11] needs (in this case, the need to cut back or delay payment for a necessity such as food, medical care, or housing); and receipt of a means-tested benefit (Medicaid, AFDC, food stamps, or housing subsidies). The degree of over- or underrepresentation is measured for several different activist groups: voters, campaign workers, campaign contributors, contactors, community activists, members of local boards, and protesters. By varying both the *characteristics* of activists and the *activities* through which they can be over- or underrepresented, Figure 2 tells us something about both parts of our puzzle:

**FIGURE 2**

**Representation of Liberal Attitudes, Financial Pinch, Receipt of Means-tested Benefits, and Socio-economic Disadvantage: Logged Representation Scores by Type of Activists**



*Note*: Voting = voted in 1988 presidential election; contacting = contacted an official in the past year; protest = took part in a protest in past two years; community activity = took part in informal community activity or attended a community meeting in past year; board member = served in past two years; campaign work = worked during 1988 campaign; campaign giver = gave a contribution during 1988 campaign.

what characteristics of citizens are better represented through activity and what activities better represent citizen characteristics.[12] It also tells us something about the extent of the under- and overrepresentation. For instance, the top line of the figure tells us that among voters, liberals are represented proportionately to their representation in the population as a whole, those who felt a financial pinch are somewhat underrepresented, and those who receive means-tested benefits are the most underrepresented among voters.

For all kinds of activities, participants represent most accurately the attitudes of individuals as measured by the standard questions. Although economic liberals are twice as likely to have reported attending a protest, they are otherwise represented proportionally among activists. However, when we consider the representation index for differences based on the actual needs of citizens and, even more when we consider the index for differences based on receipt of welfare benefits, we find that the disparities are much greater. There are also significant variations in terms of types of activity. (Figure 2 lists the activities roughly in order of the extent of the disparity between the sample as a whole and the activist population.)

For various activities associated with elections, the voting population and the population of campaign workers are, as we have mentioned, representative of the population at large with respect to attitudes and only somewhat unrepresentative of those who have had to cut back on necessities. However, those who receive means-tested benefits are substantially underrepresented among voters and, especially, among campaign workers. Among campaign contributors, the underrepresentation of those who have had to cut back on necessities or who receive welfare is most pronounced. This is hardly surprising. We would not ordinarily expect those who have severe economic problems to be campaign contributors. But that does not change the substantive implication of that fact for the messages that are received through participatory channels—in this case, one of the most effective and salient channels. Compared with campaign contributors, the underrepresentation of those who have had to cut back or those who receive means-tested benefits in other activist populations is more moderate. The differences, however, are still substantial compared with the representation of policy attitudes.

For three modes of activity that might be particularly relevant for needy citizens (getting in touch with public officials, attending protests and demonstrations, and being active in the community) there is a slight degree of underrepresentation of those who have felt financially strapped. But for those who receive means-tested benefits, the underrepresentation is more severe. Contacting is, presumably, especially important for citizens who receive government benefits, since ensuring the flow of benefits may entail the need to deal with officials. Recipients of means-tested benefits, however, are substantially underrepresented among the contactors. As the form of activity that is often described as "the weapon of the weak," protest is also relevant for disadvantaged groups that lack financial resources or connections; but those receiving a means-tested benefit are underrepresented in that participant population, as well. And the recipients of means-tested benefits are about half as likely to be community activists as their proportion in the population would warrant.

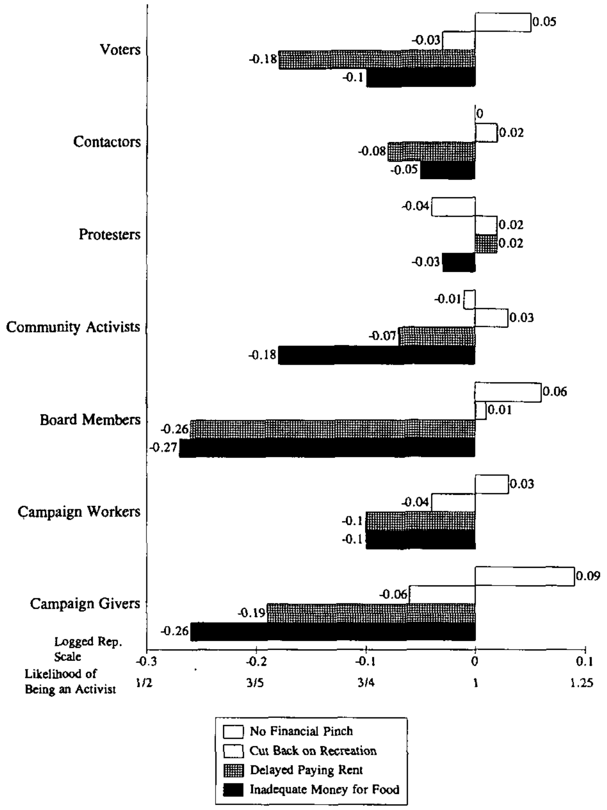
Political acts vary with respect to the number of people who perform them. We might have expected that the smaller the number of people who have engaged in a particular form of participation, the less representative the activist group. Although voters are both the most numerous and the most representative of the activist groups, overall, the pattern does not hold. Much larger proportions of the population reported making campaign contributions than sitting on local boards or attending protests, but the contributors deviate from the population as a whole much more than do the board members or the protesters.

### Severity of Financial Need

As Figure 2 shows, those reporting a financial pinch are not as severely underrepresented in political activity as are those who receive means-tested benefits or those who are in the socio-economically disadvantaged category. This is probably the case because those who report such a financial pinch may, given our questions, be reporting a more episodic and less basic economic deprivation. They are a larger proportion of the population than are the other two groups. It is useful to look more closely at those who have cut back on spending in order to refine our analysis of the representation of various participant groups. Not all citizens who have to cut back on spending are needy—at least, not equally needy. In Figure 3 we compare four groups with different levels of economic strain: those who made no cutbacks in expenditures (even for recreation), those who cut back on recreation but nothing else, those who delayed rent or house payments, and those who cut back on the amount or quality of food purchased. (Since the differences between the financially pinched and the population as a whole are not as great as those for receipt of needs-tested benefits, we have expanded the horizontal axis in Figure 3 to make it visually clearer. The reader should note that the differences on Figure 3 are within a narrower range of variation than those on Figures 2 or 4.) Curtailing expenditures for housing or food clearly bespeaks more severe deprivation than economizing on recreation or making no cuts at all. Figure 3 shows that those who took the more drastic measures are less visible in the participant population. They are somewhat underrepresented among the voters and strikingly underrepresented among those who sit on local governing boards or take part or contribute to campaigns. Those who report a shortage of funds for food are even somewhat underrepresented among those who engage in the activity that is supposed to require the fewest resources—protest. The one exception to the underrepresentation of those with severe need is the slight overrepresentation among protesters of those who have had to delay paying their rent.[13]

FIGURE 3

**Representation of Various Levels of Need: Logged Representations Scores by Type of Activists**



## THE VOLUME OF POLITICAL ACTIVITY

Earlier, we pointed out that the vote is unique among political acts as the one for which there is mandated equality in each citizen's input. We can learn more about the representativeness of participatory input by using the volume of activity, rather than the number of individuals, as the unit of analysis. Presumably, the more active individuals are in any particular way (the more hours they spend working on a campaign, the more letters they write to public officials, the more dollars they contribute), the more visible they and their needs, problems, and preferences become to public officials.

Table 1 shows the proportion of activity (votes, hours devoted to campaign work, total protests attended, contacts with public officials, dollars donated to campaigns) that arises within different segments of the population.[14] We focus on groups differentiated by their attitudes, their receipt of means-tested benefits, and their circumstance of socio-economic advantage or disadvantage. The differences among groups and types of activity are noteworthy. Once again, there is little distortion with respect to attitudes as expressed in response to survey items. With one exception, liberals and conservatives on economic issues are represented roughly proportionally with respect to the volume of input for various kinds of participation. The exception is protest: liberals attend more than their share, conservatives, less than their share, of protests. With respect to the disadvantaged and those who receive means-tested government benefits, the pattern is also familiar. We have already seen that regardless of the form of participation, those who are less well off by a variety of measures are less likely to take part than are those who are better off. When it comes to the amount of participatory input, they are also underrepresented more or less uniformly across activities.

There are, however, striking differences among the various participatory acts, differences that reflect whether the underrepresentation derives simply from the fact that the economically disadvantaged

**TABLE 1**

**Proportion of Votes, Campaign Money, Contacts, and Protests from Various Population Groups**

| | SOCIO-ECONOMIC STATUS | | ATTITUDES | | BENEFITS | |
|---|---|---|---|---|---|---|
| POLITICAL ACTIVITIES | ADVANTAGED | DISADVANTAGED | CONSERVATIVE | LIBERAL | NON-MEANS-TESTED | MEANS-TESTED |
| Percentage of sample | 17 | 19 | 38 | 30 | 29 | 9 |
| Votes[a] | | | | | | |
| Activists | 26 | 13 | 38 | 34 | 30 | 9 |
| Activity | 26 | 13 | 38 | 34 | 30 | 9 |
| Campaign work | | | | | | |
| Activists | 36 | 6 | 36 | 40 | 27 | 7 |
| Activity | 31 | 7 | 33 | 43 | 30 | 10 |
| Contributions | | | | | | |
| Activists | 40 | 4 | 40 | 36 | 27 | 4 |
| Activity | 65 | 1 | 42 | 35 | 25 | 2 |
| Contacts | | | | | | |
| Activists | 30 | 7 | 38 | 36 | 28 | 7 |
| Activity | 34 | 6 | 38 | 36 | 27 | 6 |
| Protests | | | | | | |
| Activists | 30 | 2 | 28 | 46 | 20 | 7 |
| Activity | 31 | 2 | 25 | 52 | 22 | 6 |
| N | 425 | 469 | 945 | 761 | 732 | 227 |

[a] The principle of one person, one vote dictates that the proportion of activists and activity will be the same for voting.

are less likely to become activists or whether, once active, they also do less (write fewer letters, give less time or money to a campaign, etc.) than activists who are better off. We can differentiate activities in terms of whether they require an investment of money or of time and, if the latter, whether they are performed sporadically or require a commitment of time on a more sustained basis even if over only a short period.

When we compare the proportion of activists that come from any particular group with the proportion of total activity that arises from that group, we find that with respect to activities that require time, the underrepresentation of the disadvantaged seems to spring from the fact that they are less likely to be active. Once active, they do not do less. Indeed, with respect to an activity that requires time on a more sustained basis, getting involved in a campaign, the disadvantaged actually give more hours than activists who are better off. For example, those who receive means-tested benefits constitute 9% of the sample but only 7% of those who worked in a campaign. However, they account for 9% of the total hours volunteered in campaigns.

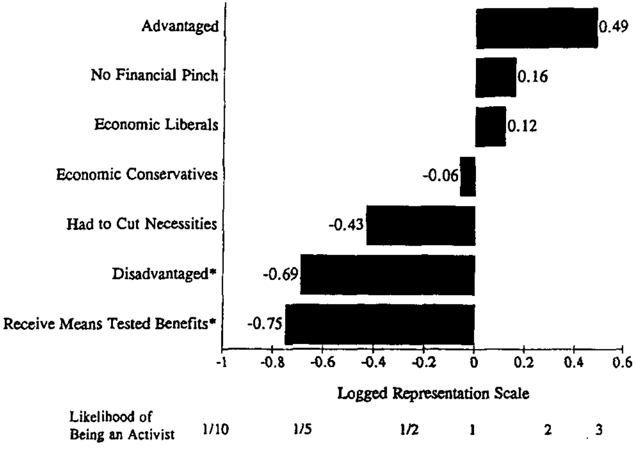
When it comes to total input of money, however, the pattern is very different. Not only are the disadvantaged less likely to make contributions but (not surprisingly) their donations are much smaller when they do. Thus, the 9% of the sample that receives means-tested government benefits make up only 4% of the contributors and a mere 2% of the total dollars contributed. Similarly, those with family incomes below twenty thousand dollars and no education beyond high school are 19% of the respondents but 4% of the campaign donors. They account for only 1% of all dollars. In contrast, those with incomes over fifty thousand dollars and at least some college education constitute 17% of the respondents but fully 40% of the contributors. Their donations make up nearly two-thirds (65%) of all funds collected.

## SENDING A MESSAGE

Participatory acts differ not only in the extent to which their volume can be multiplied but also the extent to which they can convey to policymakers detailed information about citizen concerns. Our data allow us to investigate in several ways the communications that accompany political activity. Let us begin by considering one group of activists who join an activity of substantial dimensions with an explicit message. The group is those who contributed $250 or more to a political campaign and who reported that they "communicated to the candidate or to someone involved in running the campaign [their] views on an issue of public policy—for example, about what [they] wanted the candidate to do when in office." Figure 4 indicates how well various subgroups defined by their attitudes and socio-economic circumstances are represented within a group of participants whose activity is high in its potential both for generating pressure and conveying information to candidates. As usual, those with liberal or conservative

FIGURE 4

**Representation of the Big Campaign Givers who Communicate an Explicit Message**



* The values for the disadvantaged and those that received means tested benefits are only for big campaign givers and not big givers who communicated an explicit message. There were no big givers who communicated a message among the disadvantaged and those that received means tested benefits. The LRS for these groups would be negative infinity.

policy preferences with respect to government provision of jobs or services are roughly proportionally represented among those who make major campaign donations. In contrast, there are substantial deviations from proportionality when it comes to actual circumstances. The advantaged are nearly three times as likely—those who have had to cut back on necessities, roughly half as likely—to appear in this politically potent group as is their proportion of the population. The most marked underrepresentation is found among the disadvantaged and those in households where a family member receives a means-tested benefit.

### Political Activity and Program Participation

We have shown that those receiving such means-tested benefits as AFDC, Medicaid, food stamps, and subsidized housing are much less active than those receiving such non-means-tested benefits as Social Security, veterans' benefits, or Medicare. We can take the analysis one step further, however, and probe whether these activities are in any way directly connected to these benefit programs and whether, therefore, the government receives more messages about benefits programs from recipients of non-means-tested benefits than from recipients of means-tested benefits.

In our survey we inquired whether recipients of various government benefits had been active *in relation to* that benefit. For each government program for which the respondent was a recipient, we asked: Had they taken that program into account in deciding how to vote? Had they given a campaign contribution based, at least in part, on concern about it? Had they contacted an official to complain about the program? Did they belong to an organization concerned about that program? (This battery of questions was asked of all respondents who indicated that they, or any family member in the household, received a particular government benefit.) Figure 5 presents data on the percentage of the recipients of each benefit who report an activity related to the benefit program. The data are consistent with what we know about the overall activity levels of the recipients of government benefits. For each kind of activity (voting, contributing, contacting, and being a member of an organization) recipients of non-means-tested benefits are more likely to have been active than recipients of means-tested benefits.

The difference is especially striking with respect to membership in an organization associated with the benefit program (with the American Association of Retired Persons and veterans' organizations presumably playing a major role) and with campaign donations. However, it applies, as well, to the considerations that enter into voting decisions.[15] The data on contacting are interesting. We might expect that inclusion in the non-means-tested programs would be more or less automatic and so would require fewer contacts. Nevertheless, Medicare recipients are more likely than Medicaid recipients, and Social Security recipients, more likely than AFDC recipients, to contact about their benefits.[16] Clearly, the government hears more from those on some programs than others; and the ones it hears from are the more advantaged citizens.[17]

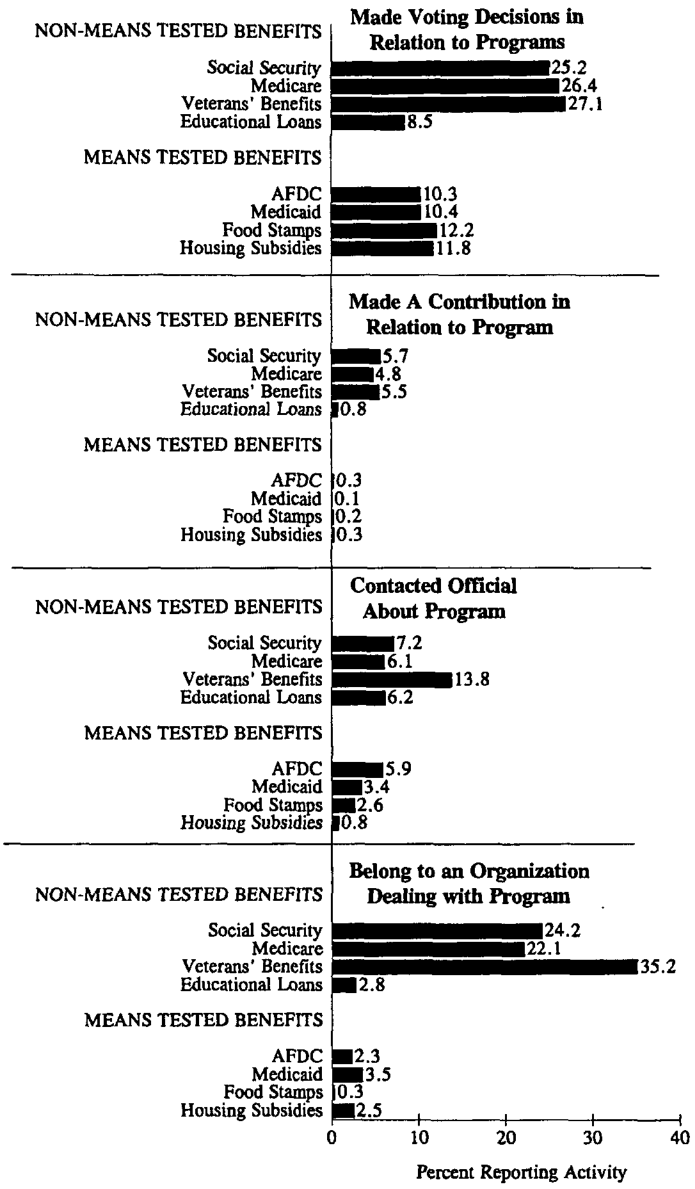
### What Do They Say?

We can extend our analysis one step further by examining directly the issue basis of citizen activity. We have probed who is active in what ways, but we have not investigated the substantive content of that activity. Let us now consider what activists actually say.

Earlier we mentioned that political activities differ in their capacity to convey explicit messages to policymakers. For example, votes are limited in their ability to transmit precise information about citizen priorities and preferences in contrast to contacts or protests. Forms of participation also vary in how far those who take part bring public policy concerns to their activity. Each time a respondent indicated having engaged in a particular activity, we inquired whether there was any particular issue or problem, "ranging from public policy issues to community, family, and personal concerns," that led to the activity. Across the totality of more than thirty-seven hundred political acts discussed by our respondents, in 62% of cases, respondents provided a comprehensible, "codable" answer about the policy concerns that animated the activity. Considering some of the acts separately, we find that only 43% of voters (in contrast to 82% of those active in their communities, 87% of contactors, and 95% of protesters) cited at

FIGURE 5

Activities Directly Related to Benefit Programs



least one identifiable public policy issue as the basis of their activity. By analyzing the substantive concerns behind what we shall call "issue-based activity," we can characterize more accurately the representativeness of participatory input.

Table 2 summarizes the subject matter behind the political activity in which a codable issue concern was expressed and compares advantaged and disadvantaged respondents with respect to the issue concerns that animate their participation. In order to ensure that what is on people's minds was actually communicated to public officials, we focus solely on those activities in which an explicit message can be sent: contacting, protesting, campaign work, or contributions accompanied by a communication, informal community activity, or voluntary service on a local board.[18] The issue-based political act is the unit of analysis, and the figures represent the proportion of all issue-based activities for which the respondent mentioned, among other things, a particular set of policy concerns.[19]

Although the advantaged and disadvantaged are similar in having wide-ranging policy concerns, they differ in the distribution of their concerns.[20] Compared with the issue-based activity of the advantaged, that of the disadvantaged is more than twice as likely—that of respondents in families receiving means-tested benefits, four times as likely—to have been animated by concerns about basic human needs, such as poverty, jobs, housing, and health.

**TABLE 2**

**What Respondents Say: Percentage Mentioning Particular Issues as the Subject of Their Activity (Information-rich Activities Only)**

| | POPULATION GROUPS | | | |
|---|---|---|---|---|
| ISSUES | ALL | DISADVANTAGED | ADVANTAGED | RECEIVES MEANS-TESTED BENEFIT |
| Basic human needs | 10 | 21 | 8 | 32 |
| Economic issues | 10 | 5 | 12 | 9 |
| Social issues | 10 | 6 | 12 | 9 |
| Education | 12 | 10 | 15 | 18 |
| Environment | 9 | 2 | 8 | 2 |
| Crime or drugs | 9 | 10 | 6 | 8 |
| Foreign policy | 3 | 0 | 3 | 0 |
| N of respondents | 2,517 | 480 | 425 | 228 |
| N of issue-based acts | 1,556 | 123 | 432 | 73 |

*Note*: Information-rich acts are those in which an explicit message can be sent to policy makers: contacting, protesting, campaign work or contributions accompanied by a communication, informal community activity, and voluntary service on a local board. The numbers in the cells represent the proportion of such acts having identifiable issue content in which there was a reference to the particular issue.

Moreover, their activity is more likely to have been motivated by concern about drugs or crime. The activity of the advantaged, in contrast, is more likely to have been inspired by economic issues such as taxes, government spending, or the budget or by social issues such as abortion or pornography.

When we consider the actual number of communications, however, a very different story emerges. Because the disadvantaged are so much less active than the advantaged, public officials actually hear less about issues of basic human need from the disadvantaged as from the slightly smaller group of advantaged respondents, even though references to basic human needs occupy relatively greater space in the bundle of communications emanating from the disadvantaged.[21]

These findings might suggest that although the disadvantaged are underrepresented with the respect to participatory input, their concerns and needs are, nonetheless, being expressed by others. When the disadvantaged speak for themselves on issues of basic human need, however, their communications differ in two fundamental ways from those sent by others. First, when the disadvantaged communicate with public officials about basic human needs, they are much more likely than the advantaged to be concerned about problems that affect them personally. Even affluent citizens may have need for government assistance with respect to basic human needs. They may have health problems or a handicapped child in school. If elderly, they receive Medicare and Social Security. Still, a much larger proportion of the messages from the disadvantaged about basic human needs involve particularized communications about problems specific to themselves or their families: a question about eligibility for Social Security, a complaint about the conditions in a housing project, a request by a disabled respondent for special transportation. (These are actual examples from our data.)

Such particularized concerns were behind fully 56% of the issue-based activity in which human needs issues were mentioned by the disadvantaged but only 8% of the activity in which they were mentioned by the advantaged. Even when the human needs issue was framed as a policy issue, rather than a particularized concern, the disadvantaged were much more likely to report that the problem was one that affected themselves or their families, as well as others in the community. When discussing basic human needs policy issues, 34% of the disadvantaged and 23% of the advantaged indicated that the issue affected them as well as others. Taken together, of those who communicated to public officials about issues of basic human needs, 71% of the disadvantaged but only 29% of the advantaged were discussing something with an immediate impact upon themselves or their families. It is axiomatic in the literature on lobbying that public officials listen more carefully to self-interested advocates who are affected by the policies they discuss. Presumably, the analogous principle applies to communications from individuals: stories about basic human needs sound different to policymakers when told by those who are in need.

The responses of advantaged and disadvantaged differ even more fundamentally when it come to their actual content. So far, we have concentrated solely on the subjects people talk about without considering what they actually say. The appropriate governmental role in addressing problems of basic human need is an issue about which liberals and conservatives disagree deeply. In Table 3 we differentiate advantaged and disadvantaged respondents in terms of their liberalism or conservatism on economic issues. Overall, the activity of disadvantaged liberals is the most likely to be animated by concerns about issues of basic human needs. Among the advantaged, there is almost no difference between the ideological groups in terms of the proportion of their activity that is inspired by such concerns. However, the activity of

**TABLE 3**

**What Liberal and Conservatives Say: Percentage Mentioning Particular Issues as the Subject of Their Activity (Information-rich Activities Only)**

| | DISADVANTAGED | | ADVANTAGED | |
|---|---|---|---|---|
| ISSUES | LIBERALS | CONSERVATIVES | LIBERALS | CONSERVATIVES |
| Basic human needs | 21 | 17 | 10 | 8 |
| Economic issues | 7 | 7 | 7 | 17 |
| N of respondents | 168 | 125 | 98 | 219 |
| N of issue-based acts | 45 | 38 | 126 | 199 |

*Note*: Information-rich acts are those in which an explicit message can be sent to policy makers: contacting, protesting, campaign work or contributions accompanied by a communication, informal community activity, or voluntary service on a local board. The numbers in the cells represent the proportion of such acts having identifiable issue content in which there was a reference to the particular issue.

advantaged conservatives is the most likely to spring from concerns about economic issues. Given that there are more than twice as many conservatives as liberals among the advantaged, a majority of the activity from the advantaged on issues of basic human need comes from conservatives. Given that the advantaged are so much more active than the disadvantaged, activity inspired by concerns about basic human needs on the part of advantaged conservatives far outweighs such activity on the part of disadvantaged liberals.

Since the largest share of activity animated by concerns about basic human needs emanates from advantaged conservatives, we should examine just what is said by the activists. Some of the policy statements about human needs clearly take a liberal point of view. Various respondents discussed the issue basis for their activity in terms of, for example, concern about "insufficient effort and expenditure to deal with poverty and other social problems," "a bill to help the homeless," and "the needy people in the United States regardless of race, color, or creed." Other statements were as identifiably conservative. For example, one respondent asserted that "welfare should be done away with." Another discussed protesting against a Puerto Rican family moving into the neighborhood. Still another got involved in a campaign out of a "dislike of big government, welfare state, and big brothers." Not all the policy statements about issues of basic human need could be so readily categorized. Unadorned references to "Social Security," "housing," or "health issues" do not reveal the direction of respondents' preferences.

Close reading of the verbatim responses shows marked differences between what liberals and conservatives say when they discuss policy about basic human needs. Consistent with figures given earlier, among disadvantaged liberals a substantial share—and among disadvantaged conservatives nearly all—mention of basic human needs in conjunction with issue-based activity involves particularized communications. The remaining statements by disadvantaged liberals are divided evenly between those that are not identifiably liberal or conservative and those that are clearly liberal; the few remaining statements by disadvantaged conservatives are ambiguous. Thus, to the extent that disadvantaged respondents, regardless of ideology, made policy statements about basic human needs in association with political activity, none of those statements urge reduced public attention to issues of basic human need.[22]

Among the advantaged, verbatim responses about basic human needs are both more numerous and more likely to focus on policy issues, rather than particularized concerns. Among the liberals, brief recitations of issues without obviously liberal or conservative content account for one-third of the statements. The remaining two-thirds are unambiguously liberal. For advantaged conservatives, the pattern is very different. Half of their issue statements have no identifiable ideological direction, and the other half are divided evenly between clearly liberal and clearly conservative content.

In short, with respect to concerns about basic human needs, information-rich forms of participation carry different kinds of messages from different groups. The issue-based activity of the disadvantaged and recipients of means-tested government benefits is more likely to be inspired by such concerns. However, because these groups are less active, they actually send fewer messages about basic human needs than do more advantaged respondents. The communications from the advantaged differ in two fundamental ways from those sent by the disadvantaged. First, the advantaged are much less likely to be discussing problems that they experience in their own lives. In addition, policy statements from the advantaged are not uniformly supportive of greater efforts to meet problems of basic human needs.

## CONCLUSION

We have investigated the consequences for the representativeness of participatory input of the substantial differences among Americans in their propensity to take part. Our analysis has shown that although similar in their preferences as measured by standard NES attitude questions, citizens who are active and those who are not are quite different in their demographic attributes, their economic needs, and the government benefits they receive. These disparities are exacerbated when we move from the most common political act, voting, to acts that are more difficult, convey more information, and exert greater pressure. With respect to the volume of activity, the disparity is especially great for electoral contributions: the advantaged account for the overwhelming share of the dollars donated to campaigns.

When we examined the issues that animate political participation, we found that while all groups bring diverse concerns to their activity, the particular mixture differs substantially among groups. In terms of a set of issues of particular salience to those who are economically disadvantaged, namely, concerns about basic human needs, the disadvantaged speak with a distinctive voice. Not only do such issues weigh more heavily in their lists of concerns, but when they communicate about these matters to public officials, they are more likely to be discussing issues that touch their own lives and more likely to be prescribing greater public attention to these needs.

If those who take part and those do not were similar on all politically relevant dimensions, then substantial inequalities in participation would pose no threat to the democratic principle of equal protection of interests. As our analysis has demonstrated, this is hardly the case. Those whose preferences and needs become visible to policymakers though their activity are unrepresentative of those who are more quiescent in ways that are of great political significance: although similar in their attitudes, they differ in their personal circumstances and dependence upon government benefits, in their priorities for government action, and in what they say when they get involved. In short, it does matter who participates.

## APPENDIX A: CITIZEN PARTICIPATION SAMPLE

Our data come from a two-stage survey of the voluntary activity of the American public. The first stage consisted of over 15,000 telephone interviews of adult Americans conducted by the Public Opinion Laboratory of Northern Illinois University and the National Opinion Research Center (NORC) during the last six months of 1989. These screener interviews, which were between 15 and 20 minutes in length, provided a profile of political and nonpolitical activity, as well as basic demographic information. At this stage respondents were selected randomly from phone exchanges matched to the primary sampling units of the NORC national, in-person sampling frame. This clustered phone sample was designed to be representative of the American population. Within each household, adults were chosen at random using a Kish table. To select respondents for a second stage of in-person interviews, the sample of 15,000 was first reweighted to adjust for the fact that the screener had yielded a disproportionate share of women. The sample was then stratified by race and ethnicity (black, Latino, and "all other") and by level and type of political participation. Blacks, Latinos, and political activists were oversampled with weights ranging from 1 for inactive Anglo-Whites to 16 for highly active Latinos. In the spring of 1990, NORC conducted in-person interviews of an average length of almost two hours each with 2,517 of the original 15,000 respondents. All data reported in this paper come from the longer reinterview.

In a complex survey design like this, the effective sample size depends not only upon the weights but also upon the specific statistical technique used, the population concerned, and the question being asked (Skinner, Holt, and Smith 1989). We have followed the standard procedure of reweighting our sample to make it representative (using the reciprocal of the sampling weights) and setting the effective sample size to 2,517. The proportion of each subgroup (e.g., blacks) in the reweighted file is therefore equivalent to their proportion in a representative sample of the population. The number of actual cases in various subgroups of the reweighted file varies beyond the difference in their proportions. Because the sample was designed to oversample activists, blacks, and Latinos, we interviewed more activists and more minorities than their weighted number and proportion in the reweighted file would indicate. Thus, using the weighted number of cases to calculate standard errors for the characteristics of these groups (such as their mean activity on Figure 1) overestimates the amount of uncertainty in our results. On the other hand, since inactive whites were undersampled, there are fewer of them in the reweighted sample than their proportion or reweighted numbers would indicate. Thus, using the reweighted number of cases for inactive whites underestimates the amount of uncertainty.

The most conservative estimate of the actual standard errors can be computed by taking the simple random sample obtained from our data after dropping from each stratum the "extra" cases that come from oversampling. When we do this, we have a simple random sample of about 1,000 embedded within our larger sample. The standard errors for this sample would be [square root(2517/1000) = 1.59] larger than what we estimate. This is an exceptionally conservative estimate. Studies of complex samples suggest that the effective sample size is much more appropriately set at the number of interviews, or 2,517 (Kish and Frankel 1974). Moreover, for many of the subgroups that we examine, such as the samples of activists on Figure 2 (except for voters), we have effective samples that are at least four times greater than the numbers obtained when we reweight to produce a representative sample. Readers of an exceptionally conservative bent might still want to inflate all standard errors by 1.59. This seems excessively cautious, however, because standard errors for participants in all activities except voting should be divided by at least a factor of 2 (square root of 4). On balance, we believe it is easiest (and not at all misleading) to use standard errors based on an assumption that we have a representative sample of 2,517.

## APPENDIX B: LOGGED REPRESENTATION SCALE

The basis of LRS can be understood by considering a two-by-two table of some act of participation by some attribute:

| | Attribute | | |
|---|---|---|---|
| Political Act | Blacks | Whites | Marginal % |
| Participants | $q_{11}$ | $q_{12}$ | P |
| Nonparticipants | $q_{21}$ | $q_{22}$ | N |
| Marginal % | B | W | — |

Here, $q_{ij}$ is the percentage of each cell in the total population; and B, W, P, and N are marginal percentages. If participation and being black are unrelated (i.e., if we have equal representation by race), then $q_{11}$ is the product of the marginals: $q_{11} = P * B$. This suggests a representation scale (RS) that is $q_{11}/(P * B)$. Values greater than 1 indicate overrepresentation of blacks relative to their proportion in the population, and values less than 1 indicate underrepresentation. This measure has an unfortunate feature: it ranges from zero (when $q_{11}$ is zero) to infinity (when $P * B$ is near zero). Taking the logarithm of $q_{11}/(P * B)$ produces the LRS ranging from minus infinity to plus infinity, with zero indicating equal representation.

The LRS has many appealing attributes. First, it has the value zero for the situation in which participation and some characteristic (e.g., being black) are unrelated. Second, the measure has some informative interpretations. The quantity $q_{11}/(P * B)$ can be written as

$$\begin{aligned} RS &= q_{11}/(P * B) = (q_{11}/B)/P \\ &= \text{probability of participation given that one is black/ Probability of being a participant} \\ &= q_{11}/(P * B) = (q_{11}/P)/B \\ &= \text{Probability of being black given you are a participant/ Probability of being black} \end{aligned}$$

The first interpretation is the ratio of the likelihood that a group member is a participant to the likelihood that any individual in the population is a participant. A ratio greater than zero indicates that group members are more likely to be participants than those in the general population and a ratio less than zero indicates the reverse. The second interpretation is the ratio of the likelihood that a participant is a group member to the likelihood that any individual in the population is a group member. A positive ratio indicates that participants are more likely to be group members than those in the general population. The equality of these measures indicates that if blacks are underrepresented among participants, then participants are just as underrepresented among blacks. Finally, the measure provides a way of indexing cells in two-by-two tables according to their representativeness; and it uses all three independent pieces of information in any two-by-two table.

## APPENDIX C: QUESTIONS USED IN THIS PAPER

### Activities

1. In talking to people about elections, we find that they are sometimes not able to vote because they're not registered, they don't have time, or they have difficulty getting to the polls. Think about the presidential elections since you were old enough to vote. Have you voted in all of them, in most of them, in some of them, rarely voted in them, or have you never voted in a presidential election?
2. Thinking back to the national election in November 1988, when the presidential candidates were Michael Dukakis, the Democrat, and George Bush, the Republican, did you happen to vote in that election?
3. Since January, 1988, the start of the last national election year, have you worked as a volunteer—that is, for no pay at all or only for a token amount—for a candidate running for national, state, or local office?
4. Since January, 1988, did you contribute money—to an individual candidate, a party group, a political action committee, or any other organization that supported candidates?
5. In your best estimate, about how much money in total did you contribute since January, 1988? Just give me the letter from this card. [$50 or less, $51–$100, $101–$250, $251–$500, $501–$1,000, $1,001–$2,500, more than $2,501]
6. During the campaign, did you communicate to the candidate or to someone involved in running the campaign your views on an issue of public policy—for example, about what you wanted the candidate to do when in office?
7. In the past twelve months, have you initiated any contacts with a *federal elected* official or someone on the staff of such an official: I mean someone in the White House or a Congressional or Senate Office?
8. What about a non-elected official in a *federal government agency*? Have you initiated a contact with such a person in the last twelve months?
9. What about an *elected* official *on the state or local level*—a governor or mayor or a member of the state legislature or a city or town council—or someone on the staff of such an elected official?
10. And what about a non-elected official in a *state or local government agency or board*? Have you initiated a contact with such a person in the last twelve months?
11. In the past two years, since [current month 1988], have you taken part in a protest, march, or demonstration on some national or local issue (other than a strike against your employer)?
12. Now some questions about your role in your community. In the past *two years*, since [current month 1988], have you served in a voluntary capacity—that is, for no pay at all or for only a token amount—on any official issues such as a town council, a school board, a zoning board, a planning board, or the like?
13. Have you attended a meeting of such an official local government board or council in the past *twelve months*?
14. Aside from membership on a board or council or attendance at meetings, I'd like to ask also about informal activity in your community or neighborhood. In the past twelve months, have you gotten together *informally* with, or worked with, others in your community or neighborhood to try to deal with some community issue for problem? (If you have mentioned this activity elsewhere, perhaps in connection with your church or synagogue, or an organization or local campaign, don't repeat it here.)

### Demographics/Policy Views

1. Do you consider yourself Hispanic or Latino?
2. What is your race? (Which category best describes your racial background?)
3. Did you get a high school diploma or pass a high school equivalency test?
4. What is the highest degree you have earned?
5. Which of the income groups listed on this card includes the *total* 1989 income before taxes of *all members of your family living in your home*? Please include all salaries, wages, pensions, dividends, interest, and all other income. [If uncertain:] What would be your best guess?
6. Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?
7. We hear a lot of talk these days about liberals and conservatives. Here is a scale on which the *political* views that people might hold are arranged from extremely liberal—point 1—to extremely conservative—point 7. Where would you place yourself on this scale?
8. Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Suppose these people are at one end of the scale at point number 1. Others think that the government should just let each person get ahead on his or her own. Suppose these people are at the other end, at point 7. And, of course, some other people have opinions somewhere in between at points 2, 3, 4, 5, or 6. Where would you place yourself on this scale?
9. Some people feel that the government should provide fewer services, even in areas such as health and education in order to reduce spending. (Suppose these people are at one end of the scale at point number 1.) Other people feel it is important for the government to provide many more services even if it means an increase in spending. (Suppose these people are at the other end, at point 7. And, of course, some people have opinions somewhere in between at points 2, 3, 4, 5, or 6.) Where would you place yourself on this scale?

## Notes

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1. We consider the representativeness of the participatory input from the perspective of a concern with its impact on the communication of citizen needs and preferences to political elites and thus a concern with equal protection of interests. Participation may also perform other functions (e.g., legitimating a regime, increasing popular commitment to democratic values, developing the capacities of those who take part, and enhancing their self-esteem). On these themes, see Bachrach 1967; Barber 1984; Mansbridge 1980, chap. 17; Parry 1972; and Pateman 1970. With respect to voting in particular, see Bennett and Resnick 1990.

2. For discussion of the literature on citizen activity and extensive references, see Bennett and Bennett 1986; Conway 1991; and Milbrath and Goel 1977.

3. The literature on representation is voluminous. The classic theoretical statement is Pitkin 1967. There are several noteworthy studies of delegates to presidential nominating conventions, namely, Jackson, Brown, and Bositis 1982; Jackson, Brown, and Brown 1978; Kirkpatrick 1976, esp. chap. 10; and Miller and Jennings 1986.

4. With respect to presidential elections, data and conclusions similar to Wolfinger and Rosenstone's are contained in Schaffer 1982. Using data from several elections (1984, 1986, and 1988), Bennett and Resnick modify the Wolfinger and Rosenstone conclusion somewhat, finding that although nonvoting "does not skew most foreign policies" and nonvoters "are not more egalitarian than voters, or more hostile to business, or more in favor of extensive government ownership and control of key industries," nonvoting "does have an impact on some domestic policies, especially spending on welfare state programs" (1990, 799). Calvert and Gilchrist (1991) draw similar conclusions. All authors agree, however, that the impact of nonvoting per se does not skew the policy debate very much. Nevertheless, Bennett and Resnick add that while "it may be true that nonvoters' and voters' opinions on many political issues are indistinguishable, it is equally true that large numbers of nonvoters grant enhanced importance to the small number of intense activists" (1990, 800). There is evidence that nonpresidential elections may not adhere to these patterns. In primaries, which engage a smaller and more politicized group than general elections, differences between voters and nonvoters on policy questions seem to be more pronounced. According to Ranney voters in primaries tend to have more extreme preferences than the public at large (1975, 129).

5. While acknowledging the impossibility of imputing political preferences to those who might be assumed to share political concerns, students of organized interests demonstrate that certain kinds of interests are especially likely to achieve representation in pressure politics and that certain groups with potential political interests (e.g., the unemployed, homemakers, and welfare recipients) are not represented at all among the organizations active in Washington. Just because a particular interest achieves strong representation by a well-endowed organization does not, of course, mean that it will necessarily prevail in political controversies. However, while vigorous advocacy by an organization cannot guarantee political victory, it never seems to hurt; and absence from the fray is a disadvantage. The seminal statement of this point of view is Schattschneider 1960. More recent considerations of these matters include McFarland 1992; Salisbury 1991; and Schlozman and Tierney 1986.

6. There are many linkage studies that combine data about constituents and representatives. On the disputes in the measurement of linkage see, e.g., Achen 1977 and Eulau and Karps 1977. Erikson and Wright 1989 provide citations to the many studies that find a positive relationship between constituent characteristics and preferences and the roll-call behavior of legislators. Other studies, using a variety of approaches, demonstrate the responsiveness of elected officials to the views of active constituents, especially activists who have supported them. See, e.g., Bullock and Brady 1983; Fenno 1978; Huddleston 1984; Markus 1974; Mayhew 1974; and Verba and Nie 1972. Although these studies are suggestive for the issues raised here, they do not provide a definitive characterization of the characteristics (attitudes, economic circumstances, demographic characteristics) that officials are likely to note when they observe their constituency and try to infer its needs and preferences.

7. Note that the referent in the questions about government benefits was the respondent or any immediate family member living in the household. In the text we shall, in the name of parsimony, refer frequently to "food stamp recipients" or "those who receive veterans' benefits" when, in fact, it may be another family member in the household who receives the benefits. Also note that although there is a means test for student loans, we are categorizing them with non-means-tested benefits. The level of permissible income for student loans is much higher than for other means-tested benefits such as food stamps or AFDC, and the beneficiaries are not located primarily among the poor.

8. The scale includes the following activities, with proportions engaging in them in parentheses: voting in the 1988 presidential election (70%); working in a campaign in the 1988 election cycle (8%); making a campaign contribution in the 1988 election cycle (24%); contacting a government official within the past year (34%); attending a protest, march, or demonstration within the past two years (6%); working informally with others in the community to deal with some community issue or problem within the past year (25%); and serving in a voluntary capacity on a local governing board or attending meetings of such a board on a regular basis within the past two years (3%).

9. Unfortunately, we did not screen for salience when we asked respondents about policy issues or ask respondents to list the issues that are most important to them. Similarities or differences on such measures would be relevant to the question of the representativeness of participatory input. We shall, however, report data about the issues that animated political activity and the actual messages that activists send.

10. We define liberals and conservatives by responses to two seven-point NES items—policy view questions 8 and 9—listed in Appendix C. Liberals are those who were either on the liberal side of the scale on both items or on the liberal side on one item and in the middle on the other. Conservatives are those who were either on the conservative side of the scale on both items or on the conservative side on one item and in the middle on the other.

11. We define the advantaged as those with at least some college education and a family income over fifty thousand dollars. We define the disadvantaged as those with no education beyond high school and family incomes below twenty thousand dollars. These are groups of roughly equal size representing about one-sixth of the sample each.

12. In order to move through a large amount of data, we present, in Figure 2, the representation index for those who take a liberal position on policy and those who manifest some economic need or receive some welfare benefit from the government. The argument would not be any different if we had looked at the other side of the divide—those with conservative views and those who do not receive such benefits.

13. It was suggested to us that rent strikes were responsible for the slight overrepresentation of those who delayed paying rent or house payments among protesters. We asked all those who reported attending a protest or demonstration what the issue was. None of those who delayed rent or house payments mentioned anything related to housing as the issue at stake in the protest.

14. We asked our respondents not only whether they were active in a particular way but, if active, how much they did. For voting, of course, each voter is responsible for only one vote. We asked those active in campaigns or community politics the average number of hours per week devoted to the effort. We inquired about the precise number of protests but not about the number of contacts. However, we did ask about getting in touch with four different kinds of public officials, thus creating the opportunity for up to four contacts. Finally, we queried respondents about the total amount given to political campaigns during the 1988 campaign year.

15. It has been suggested to us that the seeming salience of entitlement programs for voting decisions is only a reflection of the fact that political leaders have drawn attention to these programs in campaigning and thus tells us more about political leaders than about citizens. However, the fact that candidates make promises about protecting Social Security or veterans' benefits but not about means-tested benefits may be a reflection of what they are hearing from citizens and the organizations to which they belong.

16. Student loans are an exception. There is relatively little activity in relation to student loans. However, there is still more activity in relation to student loans than in relation to most means-tested programs.

17. One alternative explanation might be that the recipients of such non-means-tested benefits as Medicare and Social Security are elderly. For the elderly, certain private strategies for dealing with economic difficulties (e.g., seeking additional employment) are less feasible, putting a premium on political activity as a means of coping with problems. However, if we control for age and compare the level of activity between the recipients of the two kinds of benefits, a disparity remains, as the following table of mean activity rate among benefit recipients shows:

| Age | Non-means-tested | Means-tested |
|---|---|---|
| Under 40 | 1.8 | 1.4 |
| 41–65 | 2.2 | 1.4 |
| Over 66 | 2.0 | 1.5 |

The difference between the activity rates of the two types of benefit recipients is a function, in part, of education. Those receiving means-tested benefits are, on average, less well educated than those receiving non-means-tested benefits. Controlling for education, we find that among those with no college education, the recipients of means-tested benefits are less active than the recipients of the non-means-tested benefits across all four activities measured on Figure 5. However, among those with at least some college, the two types of benefits recipients are similar in their likelihood of contacting, or in the likelihood that they will take into account the position of, a candidate on the benefit program when deciding how to vote. The means-tested recipients in this educational group are, however, still substantially less likely to make a campaign contribution or the belong to an organization related to the program. Our point is not, however, to argue that receipt of means-tested benefits causes low levels of activity but, rather, to demonstrate that a group that is dependent upon the government is, by virtue of its lack of education or any other factors, less active in expressing its concerns and therefore less visible to public officials.

18. Thus, we omit voting, attending meetings of a local board on a regular basis, and campaigning for (or contributing to) a candidate or electoral organization when the activity is not accompanied by an explicit message.

19. It should be noted that the categories in Table 2 are not exhaustive. Issue concerns ranging from gun control to local economic development have been omitted from the table. If the universe of issue concerns had been included, the figures in each column would add to more than 100%. A single political act is often inspired by more than one issue concern. The contactor who expressed concern about "public housing, teenage pregnancy, and the child care bill" would have been coded as mentioning three separate issues.

20. In coding the open-ended responses we created over 60 relatively narrow categories. In analyzing the data, we have combined these narrow categories in various ways. The components of the categories in Table 2 are as follows:

> *Basic human needs.* Various government benefits (welfare, AFDC, food stamps, housing subsidies, Social Security, Medicare, and Medicaid); unemployment (either as an economic issue or in terms of the respondent's own circumstances); housing or homelessness; health or health care; poverty or hunger; aid to the handicapped or handicapped rights.
>
> *Economic issues.* Local or national economic performance; taxes; inflation; budget issues or the budget deficit; government spending; other economic issues.
>
> *Social issues.* Traditional morality; abortion; pornography; family planning, teenage pregnancy, sex education, or contraception; school prayer; gay rights or homosexuality.
>
> *Education.* Educational issues (school reform, school voucher plans, etc.); problems or issues related to schooling of family members; guaranteed student loans.
>
> *Environment.* Specific environmental issues (e.g., clean air, toxic wastes) or environmental concerns in general; wildlife preservation; animal rights.
>
> *Foreign policy.* Relations with particular nations or to foreign policy in general; defense policy or defense spending; peace, arms control, or international human rights issues.

Note that many of the categories encompass respondents with quite different issue positions. For example, the social issues category includes many respondents who are not social conservatives such as those who are prochoice, rather than prolife, or those who are concerned about making contraceptives more easily available to teenagers.

21. That the number of issue-based acts per respondent is so much lower for the disadvantaged than for the advantaged is largely a function of their lower rates of activity. Among activists, the disadvantaged are as likely to cite issue concerns in conjunction with issue-based, information-rich activity as are the advantaged.

22. This paragraph, based on a close reading of actual verbatim responses, focuses upon policy-based activity and omits particularized activity. We do not give precise figures because we read each response as a unit without applying sample weights. Because the data are unweighted, the actual number of verbatims is higher than the numbers given in Table 3.

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# A SIGNALING MODEL OF INFORMATIVE AND MANIPULATIVE POLITICAL ACTION

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*I develop a signaling model of mass political action. I establish that rational, self-interested individuals may have incentives to engage in costly political action despite a free-rider problem. Their political actions are informative for a political leader who rationally takes a cue from the size of the protest movement. However, some information is trapped in extremist and rationally apathetic pockets of the society. Some extremists take political action regardless of their private information, to manipulate the political leader's decision. Others abstain hoping to benefit if the leader makes an uninformed decision. Rationally apathetic moderates abstain because, being nearly indifferent between the policy alternatives, they do not find it worthwhile to incur the cost of taking action. Only activist moderates take informative political action. The political leader discounts the observed turnout for extremist political action and shifts policy if the estimated number of activist moderates exceeds a critical threshold.*

Major policy shifts are often preceded by political action: people sign petitions, take part in demonstrations, or participate in violent riots to express their dissatisfaction with the status quo (Barnes and Kaase 1979; Finkel, Muller, and Opp 1989; Rochon 1990; Tarrow 1989). Neither the activists' incentives to engage in costly political action nor the incentives of political leaders to respond to protest activities are well understood. Social scientists have raised a number of theoretical and empirical questions.

First, Olson (1965) argues that political participation is plagued by a free-rider problem. The probability that one individual's action will be decisive for political outcomes is negligible in a large society. Olson concludes that rational, self-interested individuals do not have incentives to engage in costly collective action.

Second, extensive and ongoing mass participation may be an ineffective and undesirable means by which people express their policy preferences. People may be insufficiently informed about complex policy issues, perhaps rationally so (Downs 1957). People may also be limited in their ability to articulate private information that is pertinent to political decisions. On these grounds, protest activities might be interpreted as irrational, transitory frenzies of popular opinion, from which policymakers are (at least partially) isolated by the institutions of representative democracy (Schumpeter 1942).[1]

Third, it is plausible that political action might bias political decision making. The fraction of the population that is engaged in political protest is typically small. Moreover, political action is often taken by people who are not representative of the population at large. Activists tend to have more extreme policy preferences and beliefs (Finker, Muller, and Opp 1989; Nie and Verba 1975; Opp 1989). Political decisions taken in response to the pressures of extremist minorities may not be representative.

Given this assessment, it is puzzling that rational political leaders with majoritarian incentives would ever respond to political action. While they often appear to yield to the "power in numbers" (DeNardo 1985), their response seems to vary over time and across societies in a haphazard fashion. Forms of political protest that have revolutionary implications in one setting are treated as routine in another (Tarrow 1991). Tarrow concludes that social scientists are "aiming at a moving target" when analyzing collective action.

I develop a signaling model of mass political action that sheds some light on these empirical regularities and theoretical puzzles. I establish that rational, self-interested individuals may have incentives to engage in costly political action despite the presence of a free-rider problem. Their political actions are informative for a political leader who rationally takes a cue from the size of the protest movement.

First, I provide an informal exposition of the argument, in particular, explaining how the free-rider problem of political participation is partially overcome even in a large society. Then I develop a formal model and characterize the equilibrium behavior under zero, low, and prohibitively high costs of taking political action. I conclude with some possible extensions.

## OUTLINE OF THE ARGUMENT

Information is dispersed among the heterogeneous members of a society. Their private information is pertinent to the policy decision made by a political leader, who must choose between maintaining the status quo or implementing a policy alternative. The members of the society are affected by the policy and the state of the world. Consequently, the individuals' preferences over policies are a function of the state of the world.

The political leader wishes to make a decision that is advantageous for a majority of the population. Underlying this assumption is a majority rule voting mechanism that is not explicitly modeled. For example, an office-motivated leader might be more likely to remain in power if the leader's policy decisions benefit a majority. Alternatively, a community might elect a leader whose preferences over policy outcomes are identical to the median voter's.[2]

A leader fully informed about the state of the world would choose the policy preferred by a majority under the true state of the world. Traditionally, political leaders are assumed to be well informed, while the populace is said to be rationally ignorant (Downs 1957). This assumption is plausible for expert knowledge that is costly to obtain, but is less appealing for types of knowledge that are costless byproducts of practical experience. In their daily lives, individuals might obtain information about the consequences of various policies for their wealth or well-being. One individual's experience might lead to a fairly imprecise estimate of the benefits that will be derived from various policy alternatives in the future, whereas the collective experiences of all members of society might reflect these benefits quite accurately.

In this setting, each individual has some noisy information about the state of the world. In the aggregate, the population is very well informed about the mapping of policies into policy outcomes. In contrast, the political leader has no private information about the state of the world.[3] If the individuals do not reveal any of their private information prior to the leader's decision, the leader will base the decision on his or her prior beliefs about the state of the world. In many cases, the prior expectation is that the status quo is advantageous for a majority of the population. In the absence of any further information, the leader would have no reason to shift policy. A status quo bias would be built into uninformed political decision making.

Since the leader is motivated to make a policy decision that benefits a majority, individuals whose preferences are close to the median preferences would like the leader to make an informed decision. Some of these moderates expect to be better off if the leader shifted policy. They take costly political action in order to signal their private information to the leader and influence his decision. Other moderates abstain from political action even if they prefer the leader to shift policy. They are close to indifferent between the policy alternatives and do not find it worthwhile to pay the cost of taking political action to influence the leader's decision. The first group of individuals will be referred to as the *activist moderates*, the second group as the *rationally apathetic moderates*.

Some individuals with extreme preferences expect to gain if the leader were misinformed. These *anti-status-quo extremists* have incentives to take political action regardless of their private information in order to manipulate the policy decision in favor of a policy shift. Other individuals with extreme preferences expect to benefit strongly from the status quo and thus prefer the leader to make an uninformed policy decision. These *pro-status-quo extremists* abstain from political action regardless of their private information.[4]

Since the individuals are privately informed about their preferences over policy outcomes, the leader does not know the specific type (preferences and information set) of any particular individual. The leader only observes a simple aggregate statistic, the realized number of political actions. The leader cannot distinguish whether an activist is taking informative or manipulative political action. Similarly, the leader does not know the motives of any particular individual who abstains from taking political action. However, since some moderates take political action conditional on their private information, the political leader can extract some information from the aggregate number of political actions. The leader takes into account that some individuals are taking uninformative political action, while others are abstaining from political action regardless of their private information.

In equilibrium, the individuals' incentives to take political action affect the political leader's decision rule that maps the observed number of political actions into the decision to shift policy or maintain the status quo. This functional relationship, in turn, determines the individuals' incentives to take political action.

Even if activists privately incur a cost when taking political action, the free-rider problem of costly collective action is partially overcome. In establishing this result, I draw on the work of several scholars who analyze the game-theoretic foundations of voting. Their work leads to a modification of Riker and Ordeshook's (1968) argument that electoral turnout tends towards zero in a large society. In a seminal paper, Ledyard (1984) demonstrates that zero voting turnout cannot be an equilibrium for some set of strictly positive costs of voting if the alternatives are distinct and the population is of finite size. Expected voter turnout is strictly positive for some nonempty set of strictly positive voting costs. Subsequent work by Palfrey and Rosenthal (1985), however, suggests that expected voter turnout is close to zero if the electorate is large and imperfectly or incompletely informed. In the face of the empirical regularity that electoral turnout is typically large, they conclude that "we have come full circle and are once again beset by the 'paradox of not voting'" (p. 64).

I examine the case in which rational, self-interested individuals engage in costly political action. As suggested by Ledyard's analysis, the probability that one individual's political action will be decisive for the outcome is strictly positive in a finite-sized society, even if the society is very large. Thus, expected political action turnout is strictly positive for some strictly positive costs of taking political action.

Moreover, I synthesize participation and signaling games by casting political action as a signaling phenomenon. Political action informs the political leader's decisions and thus affects policy outcomes. In this setting, a small number of political actions may have a decisive effect for two reasons. First, the individuals' policy preferences are correlated. One individual's negative experience, if made public, affects other individuals' policy preferences. As a consequence, the information that is revealed through the political actions of very few individuals may convince a leader with majoritarian incentives to shift policy. Second, when extracting information from the observed number of political actions, the political leader takes into account the individuals' incentives to engage in political action. If individual incentives to engage in political action are weak, the political leader's decision rule endogeneously allows for a small number of political actions to be decisive. In contrast to voting studies, an analysis of political action does not have to explain huge turnout. On the contrary, the puzzle that requires explanation is that small numbers can often make a "big splash," as in the New York rent strikes or the Vietnam War protests (Nie and Verba 1975, 27; Lipsky 1970; Verba and Brody 1970).

In this paper, the action taken by one individual affects the utilities of all individuals, but the cost of taking action is privately incurred by the activist. A rational, self-interested individual takes political action if the expected private benefits exceed the private cost generated by the action. The expected marginal benefits are given by the private benefits that the individual expects to derive from a policy shift, multiplied by the expected probability that the action will be *decisive* for the political leader's decision. In equilibrium, an individual who takes political action is decisive with strictly positive probability. This implication of the analysis is consistent with the empirical finding that activists believe their actions to be influential (Finkel, Muller, and Opp 1989; Opp 1989).

One implication of my model is that only a subset of the population abstains from political action to avoid the associated private cost. Rationally apathetic moderates free-ride on the efforts of activist moderates. Their private information is not revealed due to the costliness of taking political action.

In contrast, the private information held by extremists is not revealed due to the distributional effects of information revelation. In their futile attempt to manipulate the political leader's decision, anti-status-quo extremists are trapped into taking costly but uninformative political action. They know that the political leader discounts the size of the protest movement to take into account that their political actions are uninformative. Given the leader's updating rule, they are "forced" to take political action to prevent an unacceptable reduction in the probability that the leader will shift policy. The abstentions of the pro-status-quo extremists are *not* motivated by the desire to free-ride and avoid the cost of taking political action. They do not have incentives to reveal their private information, because they prefer that the political leader make an uninformed decision.

Thus, information that is pertinent to political decision making is trapped in extremist or rationally apathetic pockets of the society. When the information dispersed in the population is only partially revealed, the realized number of political actions may be a misleading indicator of the state of the world. In this situation, the political leader may "mistakenly" make a decision that is disadvantageous for a majority. If the state of the world is subsequently revealed, an outside observer might come to the conclusion that uninformed frenzies of popular protest distort political decision making. And yet ex ante, the political leader's decisions are more likely to be in line with the preferences of a majority under the true state of the world if the decisions are informed by political action.

The political action of one individual creates positive or negative externalities for other individuals by affecting the likelihood of a policy shift. Self-interested individuals ignore these externalities in their political action decisions. As a consequence, the equilibrium "supply" of political action may be suboptimal in the following sense. The outcome implemented by the political action mechanism may not be the one that minimizes the median individual's expected utility or the sum of the individuals' expected utilities (Lohmann n.d.).

The activists' preferences and beliefs are not representative of the population at large. This lack of representativeness does not bias political decision making. The political leader discounts the aggregate number of political actions to take into account that some individuals have incentives to manipulate the leader's decision.

The analysis also implies that activists are heterogeneous in their estimates of the size of the protest movement and the associated probability of its success. Compared to nonactivists, they tend to believe that a protest movement is larger and more likely to succeed. This implication is consistent with several empirical findings on protest activities in Germany. Activists are found to have heterogeneous beliefs about the number of people who are engaged in, or sympathetic to, their protest; and they tend to overestimate this number (Finkel, Muller, and Opp 1989; Opp 1989).

My analysis implies that an individual may find it worthwhile to take costly political action even in a large society. The critical number of political actions that triggers a shift in policy is *endogenous*. The success of a protest movement in bringing about political change is a function of its actual size relative to its ex ante expected size. The expected size and the critical size of a protest movement depend on the individuals' incentives to participate. These incentives in turn are a function of environmental parameters that affect the benefits and costs of participation. Thus, it is not surprising that the critical size of a protest movement changes over time and differs across societies. The analysis provides a foundation for Tarrow's (1991) insight that attention should be payed to what is "*considered* disruptive, challenging and efficacious" (p. 17).

One implication of the model is that the political leader shifts policy if the political action turnout exceeds a critical threshold. Thus, the analysis provides a rational choice underpinning for models that *assume* a monotonic relationship between political action turnout and the likelihood of a shift in policy or leadership personnel (Baron 1988; DeNardo 1985).[5]

The ideas I shall develop are closely related to analyses of the information revealed through costless public opinion polls (Cukierman 1991; McKelvey and Ordeshook 1985). My analysis suggests that poll results may be "noisy" and contain a systematic bias if individuals incur a cost when responding to a poll or if the population is very heterogeneous. However, the existence of a bias does not distort policy or voting decisions if policymakers and voters rationally "undo" the bias when making inferences on the basis of poll results.

The political action mechanism characterized here does not require individuals to be well informed or articulate. It is based on the assumption that individuals costlessly gather noisy information about policy consequences in their daily lives. Moreover, the individuals may very well be limited in their ability to communicate their personal experiences and opinions on complex policy issues. I only assume that they can be physically "counted" as part of a political protest movement against the status quo. Their private information is summarized in a simple statistic, namely, the size of the protest movement. The political leader takes a cue from this simple signal.

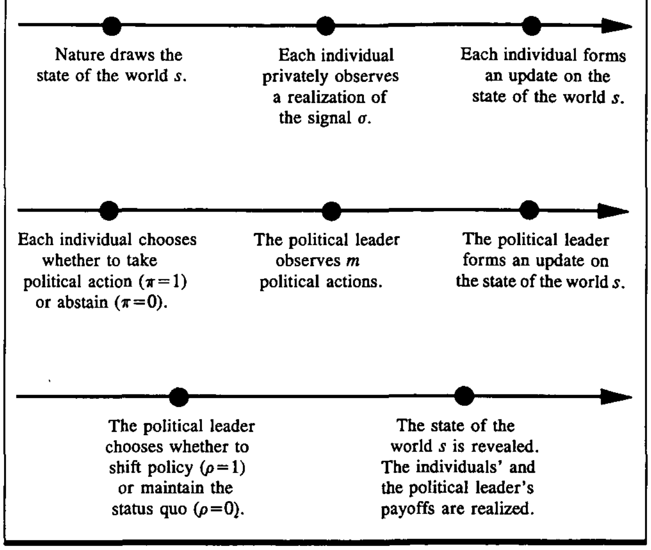
## A FORMAL MODEL

The society consists of $n$ people, indexed by $i = 1, \ldots, n$, and one political leader. The size of the population, $n$, is assumed to be large-but-finite and odd.[6] An individual $i$ has the loss function $L_i = (x - x_i)^2 + d_i c$, where $x$ is the policy outcome, $x_i$ is individual $i$'s bliss point for policy outcomes, $c$ is the cost incurred by individual $i$ when taking political action, and $d_i$ is an index variable which takes on value 1 if individual $i$ takes political action, and value 0 otherwise. For simplicity, the bliss points are assumed to be uniformly distributed between $-\bar{x}$ and $\bar{x}$, $\bar{x} > 0$. The individual bliss points are equidistant and fixed. Given this assumption, there is no uncertainty about the distribution of the individuals' bliss points.[7] The parameter $\bar{x}$ measures the heterogeneity of the population.

The policy outcome $x$ is determined by the policy $p$ and the state of the world $s$ (Gilligan and Krehbiel 1987): $x = p - s$.

The state of the world $s$ is drawn from a uniform distribution $\beta(s)$ that has strictly positive support on the unit interval.[8] The common prior on the density of the states of the world is given by $\beta(s)$. The policy $p$ is chosen from the set $\{Q, A\}$, where both the status quo $Q$ and the policy alternative $A$ are exogeneously fixed and distinct points on the unit interval, $0 < 1 - A < Q \leq 0 < A < 1$.[9] The median individual, whose bliss point is equal to zero, is indifferent between the status quo and the policy alternative for the state of the world $s = (Q + A)/2$. A majority of the population is better off under the status quo for states of the world that satisfy $s \in [0, (Q + A)/2)$ and under the policy alternative for others, $s \in ((Q + A)/2, 1]$.

FIGURE 1


Nature draws the state of the world *s*.
Each individual privately observes a realization of the signal *σ*.
Each individual forms an update on the state of the world *s*.
Each individual chooses whether to take political action (*π*=1) or abstain (*π*=0).
The political leader observes *m* political actions.
The political leader forms an update on the state of the world *s*.
The political leader chooses whether to shift policy (*ρ*=1) or maintain the status quo (*ρ*=0).
The state of the world *s* is revealed. The individuals' and the political leader's payoffs are realized.

Individuals ultimately care about policy outcomes. Since the policy and the state of the world jointly determine the policy outcome, the individuals' preferences over policies depend on the state of the world. New information that is revealed about the state of the world $s$ has the potential to allow for the implementation of a policy $p$ that is closer to the one that would be desired by the median individual if this individual were fully informed. However, the policy preferred by an individual also depends on his or her preferences over policy outcomes, summarized by the bliss point $x_i$. The revelation of information affects the likelihood that one of the policy alternatives is implemented and thus has distributional implications. Some individuals with extreme preferences over policy outcomes may expect to be worse-off if information is revealed. Finally, the specification of the cost term in the utility function implies that information revelation through political action comes at a cost privately incurred by the activists.

The political leader is motivated to minimize the loss function of the median individual, whose bliss point is equal to zero: $L_0 = x^2$. This specification reflects the leader's desire to set the policy that benefits a majority, perhaps for electoral reasons.[10] He might care about the costs of political action incurred by some activists. But these are sunk costs at the policy decision stage, so that the cost term can be omitted.

The time line is graphed in Figure 1. Nature draws the state of the world $s$. Then each individual observes an independent realization of a binary signal $\sigma$. The probability of any individual's observing the realization $\sigma = 1$ is equal to $s$, while the probability of the realization $\sigma = 0$ is given by $(1 - s)$:

$$\text{prob}(\sigma = 0) = 1 - s, \quad (1)$$

$$\text{prob}(\sigma = 1) = s. \quad (2)$$

The correlation between the binary signal $\sigma$ and the state of the world $s$ implies that the realization of $\sigma$ is informative about the state of the world.[11] Given that the individual experiences are made privately, each individual is very imperfectly informed about the state of the world $s$. In the aggregate, the population is better informed, since it observes $n$ independent draws of the signal $\sigma$. However, no single individual nor the political leader is informed about the aggregate number of individuals of type $\sigma = 1$ or $\sigma = 0$.

At the political action stage of the game, the individuals are differentiated with respect to their bliss points and information sets. Given each individual's private information at the political action stage, $i$ and $\sigma$, each chooses whether to take political action or abstain. Formally, the individuals' political action strategies are given by the probability $\pi(i, \sigma)$. An individual who takes political action ($\pi = 1$) privately incurs the cost $c$; an individual who abstains ($\pi = 0$) does not.

It is commonly understood that an individual who takes political action wishes to indicate that he or she is of type $\sigma = 1$ (equivalently, this message indicates that the individual favors a policy shift); that is, individuals can choose whether to send the costly message "I am type $\sigma = 1$" or to abstain from sending this message. The analysis of the case where the message implicit in a political action is "I am type $\sigma = 0$" (or, equivalently, "I am in favor of the status quo") is symmetric.[12]

At the policy decision stage, the political leader observes $m$ individuals engaged in political action. The leader has no private information.[13] The leader's information set at the policy decision stage contains the publicly observed number of political actions, $m$. Formally, the leader's policy decision strategy is given by the probability $\rho(m)$. Based on the public information $m$, the leader chooses whether to shift policy ($\rho = 1$) or maintain the status quo ($\rho = 0$). After the policy decision is made, the state of the world is revealed. The individuals' and the political leader's losses are realized as a function of the leader's policy decision and the state of the world.

The structure of the game is common knowledge. The equilibrium concept employed is a refinement of sequential equilibrium. The concept of sequential equilibrium (Kreps and Wilson 1982) restricts the individuals to use Bayes' Rule to update on the state of the world when they are privately informed of their type $\sigma$. Similarly, the political leader is restricted to form a Bayesian update on the state of the world after observing the aggregate number of political actions, $m$. These updates take into account the individuals' political action strategies and beliefs, the political leader's policy decision rule and beliefs, and the common priors on Nature's actions.

One characteristic of an equilibrium is that individuals do not have incentives to deviate from their equilibrium strategies. In some cases, the number of political actions that may be observed if one or more individuals deviate from their political action strategies is realized with probability 0 in equilibrium. The individual incentives to deviate will depend on their expectations about the political leader's response to an out-of-equilibrium number of political actions. However, the assumption of Bayesian rationality does not place restrictions on the political leader's out-of-equilibrium inferences. To close the model, I refine the equilibrium concept by imposing a restriction: the leader believes that the minimum number of individuals compatible with the observed number of political actions have deviated from their equilibrium strategies. Let $\underline{m}$ be the minimum, $\overline{m}$ the maximum number of political actions that can possibly be observed in equilibrium. The leader who observes a number of political actions that is admissible in equilibrium, $m \in [\underline{m}, \overline{m}]$, updates on the state of the world as if no individual had deviated from equilibrium. If the observed number of political actions is smaller than $\underline{m}$ or greater than $\overline{m}$, the leader updates his beliefs as if he had observed $\underline{m}$ or $\overline{m}$, respectively.[14] Next, I define the equilibrium.

Definition. A refined sequential equilibrium of the game is given by the individuals' political action strategies, $\{\pi(i, \sigma)\}$; the individuals' beliefs at the political action stage, $\{\beta(s|\sigma)\}$; the political leader's decision rule, $\rho(m)$; and the leader's beliefs at the policy decision stage, $\beta(s|m)$. These best responses and beliefs are consistent with each other and fulfill the following conditions:

E1. Given $\{\pi(i, \sigma)\}_{-j}$, $\{\beta(s|\sigma)\}$, $\rho(m)$, and $\beta(s|m)$, individual $j$'s political action strategy $\pi(j, \sigma)$ minimizes his or her expected loss at the political action stage, $E(L_j|\sigma)$, where the subscript $-j$ stands for "all individuals except $j$" and $E$ is an expectations operator.

E2. Given $\{\pi(i, \sigma)\}$, $\{\beta(s|\sigma)\}_{-j}$, $\rho(m)$, and $\beta(s|m)$, individual $j$ uses Bayes' Rule to update that $\beta(s|\sigma)$ is the posterior density of the state of the world $s$ conditional on the private information $\sigma$.

E3. Given $\{\pi(i, \sigma)\}$, $\{\beta(s|\sigma)\}$, and $\beta(s|m)$, the political leader's policy decision rule $\rho(m)$ minimizes his or her expected loss at the policy decision stage, $E(L_0|m)$.

E4. Given $\{\pi(i, \sigma)\}$, $\{\beta(s|\sigma)\}$, and $\rho(m)$, the political leader uses Bayes' Rule to update that $\beta(s|m)$ is the posterior density of the state of the world $s$ conditional on the public information $m$. Moreover, the political leader's out-of-equilibrium inferences are formed according to the rule $\beta(s|m) = \beta(s|\underline{m})$ if $m < \underline{m}$ and $\beta(s|m) = \beta(s|\overline{m})$ if $m > \overline{m}$.

The individuals' and the political leader's strategies and beliefs are consistent in equilibrium in the following sense. Each individual's equilibrium political action strategy $\pi(j, \sigma)$ minimizes the individual's expected loss at the political action stage, given the Bayesian update on the state of the world formed on the basis of the private information $\sigma$. The individual takes into account the other individuals' equilibrium political action strategies $\{\pi(i, \sigma)\}_{-i}$, as well as the political leader's equilibrium policy decision rule $\rho(m)$ that maps the political action turnout into a policy decision.

Similarly, the political leader's equilibrium policy decision rule $\rho(m)$ minimizes the leader's expected loss at the policy decision stage, given the Bayesian update on the state of the world formed on the basis of the publicly observed number of political actions, $m$. The leader takes into account the individuals' equilibrium political action strategies, $\{\pi(i, \sigma)\}$, that map the individuals' preferences over policy outcomes and private information into a political action decision.

## CHARACTERIZATION OF EQUILIBRIUM

I shall now present the equilibrium behavior under zero, low, and prohibitively high costs of taking political action. (The equilibrium strategies and beliefs are derived in the Appendix.) For each case, I characterize the pattern of participation, the political leader's decision rule, and the degree to which the dispersed information is aggregated.

**Proposition 1 (Zero Cost)**

1. If the cost of taking political action is zero, then individuals with moderate preferences take political action conditional on being of type $\sigma = 1$ and abstain otherwise. Regardless of their private information, all individuals with extreme preferences in favor of the policy alternative take political action, and all individuals with extreme preferences in favor of the status quo abstain.
2. The political leader shifts policy if turnout exceeds a critical threshold.
3. The information dispersed in the population is fully aggregated if the population is fairly homogeneous. Otherwise, the information held by individuals with extreme preferences is not revealed.

The individuals form beliefs about the state of the world based on their private information:

$$\beta(s|\sigma) = \begin{cases} 2(1-s) & \text{if} \quad \sigma = 0 \\ 2s & \text{if} \quad \sigma = 1. \end{cases} \tag{3}$$

Individuals of type $\sigma = 0$ tend to be more favorably disposed toward the status quo than are individuals of type $\sigma = 1$:

$$E(s|\sigma = 0) = 1/3 < 2/3 = E(s|\sigma = 1).$$

All else equal, an individual with a low bliss point will tend to favor the status quo, while an individual with a high bliss point will tend to prefer the policy alternative. Thus, individuals' incentives to take political action will depend on their private information $\sigma$ and their bliss points $x_i$.

An individual of type $\sigma = 0$ whose bliss point is exactly equal to

$$\tilde{s}_0 = (Q + A)/2 - E(s|\sigma = 0) = (Q + A)/2 - 1/3 \tag{4}$$

is indifferent between the status quo and the policy alternative. This individual does not care whether his action is decisive for the policy decision. In the absence of a strictly positive cost of taking political action, the individual is indifferent toward taking political action. The bliss point $\tilde{s}_0$ will be referred to as the political action cutpoint of individuals of type $\sigma = 0$.

Similarly, an individual of type $\sigma = 1$ whose bliss point is exactly equal to

$$\tilde{s}_1 = (Q + A)/2 - E(s|\sigma = 1) = (Q + A)/2 - 2/3 \tag{5}$$

is indifferent between the status quo and the policy alternative and thus indifferent toward taking action. The bliss point $\tilde{s}_1$ will be referred to as the political action cutpoint of individuals of type $\sigma = 1$.

The individuals' political action strategies can be summarized by a simple cutpoint rule. All individuals whose bliss points lie above their type-specific cutpoints strictly prefer to take political action; all others strictly prefer to abstain:

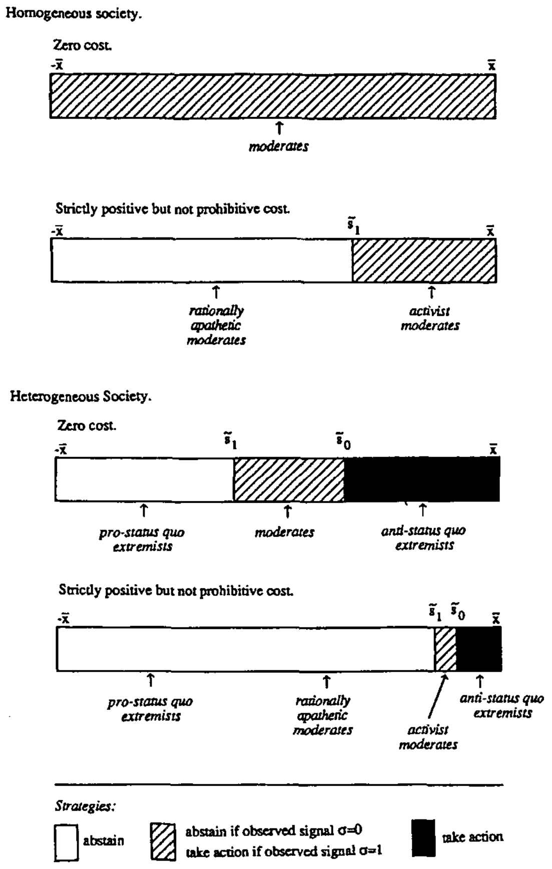
$$\pi(i, \sigma) = \begin{cases} 1 & \text{if } x_i \geq \tilde{s}_0 \text{ and } \sigma = 0 \text{ or if } x_i \geq \tilde{s}_1 \text{ and } \sigma = 1 \\ 0 & \text{otherwise.} \end{cases} \tag{6}$$

Since the posterior expectation of the state of the world formed by individuals of type $\sigma = 1$, 2/3, is greater than the expectation formed by individuals of type $\sigma = 0$, 1/3, the cutpoint $\tilde{s}_1$ lies strictly below the cutpoint $\tilde{s}_0$ [see equations 4 and 5]. Thus, individuals whose bliss points lie in the interval $[\tilde{s}_1, \tilde{s}_0)$ take political action if and only if they are of type $\sigma = 1$. The equilibrium is informative if and only if the number of individuals whose bliss points lie in this separating interval is strictly positive.

The equilibrium is fully revealing if the support of the bliss point distribution, $[-\bar{x}, \bar{x}]$, lies within the separating interval $[\tilde{s}_1, \tilde{s}_0)$ (see Figure 2). All individuals of type $\sigma = 1$ take political action, while all others abstain. The observed number of political actions, $m$, is equal to the number of individuals of type $\sigma = 1$. The political leader forms a perfect inference on the number of individuals of type $\sigma = 1$. Given the correlation between the individuals' private information $\sigma$ and the state of the world $s$ (see equations 1 and 2), the probability that $m$ individuals in a population of size $n$ are of type $\sigma = 1$ if the state of the world is given by s is equal to the binomial probability that n Bernoulli trials result in $m$ successes if the probability of a success is given by $s$, $b(m; n, s)$. Thus, the political leader forms a posterior density on the state of the world based on the number $m$:

**FIGURE 2**

**Pattern of Participation**


Homogeneous society.
Zero cost.
-x̄
x̄
moderates
Strictly positive but not prohibitive cost.
-x̄
s̃1
x̄
rationally apathetic moderates
activist moderates
Heterogeneous Society.
Zero cost.
-x̄
s̃1
s̃0
x̄
pro-status quo extremists
moderates
anti-status quo extremists
Strictly positive but not prohibitive cost.
-x̄
s̃1 s̃0
x̄
pro-status quo extremists
rationally apathetic moderates
activist moderates
anti-status quo extremists
Strategies:
abstain
abstain if observed signal σ=0 take action if observed signal σ=1
take action

*Note:*

$-\bar{x}, \bar{x}$: lower and upper support of bliss point distribution, respectively

$\tilde{s}_0, \tilde{s}_1$: bliss point of individual who observed signal $\sigma=0$ or $\sigma=1$, respectively, and is indifferent between taking political action and abstaining

$$\beta(s|m) = b(m; n, s)\beta(s)/[\textstyle\int b(m; n,s)\beta(s)\, ds]. \qquad (7)$$

The higher the number of individuals of type $\sigma = 1$, the higher is the posterior expectation of the state of the world. If the number of political actions exceeds the critical threshold $\tilde{m}$, the political leader expects the state of the world $s$ to be greater than $(Q + A)/2$; otherwise, the state of the world $s$ is expected to lie below $(Q + A)/2$. The critical threshold $\tilde{m}$ is the natural number that solves

$$E(s|m = \tilde{m}) \geq (Q + A)/2, \qquad (8)$$

$$E(s|m = \tilde{m} - 1) < (Q + A)/2. \qquad (9)$$

The political leader desires to maintain the status quo for states of the world that satisfy $s \in [0, (Q + A)/2)$ and shift to the policy alternative for others, $s \in [(Q + A)/2,1]$. Thus, the leader's policy decision rule can be summarized by a simple cutpoint rule:

$$\rho(m) = \begin{cases} 1 & \text{if } m \geq \tilde{m} \\ 0 & \text{otherwise.} \end{cases} \qquad (10)$$

The fully revealing equilibrium emerges when the conflict of interest among the members of the society is dominated by their common interest to reveal their private information in order to minimize the quadratic distance between the policy $p$ and the state of the world $s$. There exists a consensus that the informational gains to be made by informing the political leader swamp the distributional effects of the political decision.

A partially revealing equilibrium arises if the separating interval $[\tilde{s}_1, \tilde{s}_0)$ lies strictly within the support of the bliss point distribution, $[-\bar{x}, \bar{x}]$ (see Figure 2). All individuals whose bliss points lie in the interval $[\tilde{s}_1, \tilde{s}_0)$ take political action if and only if they are of type $\sigma = 1$. Individuals with bliss points above $\tilde{s}_0$ pool by taking political action regardless of their private information. Those with bliss points below $\tilde{s}_1$ also pool by abstaining from political action independently of their private information.

The political leader is aware of the different types' incentives to take political action as a function of their private information and preferences over policy outcomes. The specific type (information set and bliss point) of any particular activist or abstainer is not publicly revealed. But the distribution of the individuals' preferences over policy outcomes is common knowledge. The political leader knows that the bliss points of $\underline{m}$ individuals lie above $\tilde{s}_0$, those of $n - \overline{m}$ extremists below $\tilde{s}_1$, and those of $\overline{m} - \underline{m}$ individuals in the separating interval $[\tilde{s}_1, \tilde{s}_0)$.

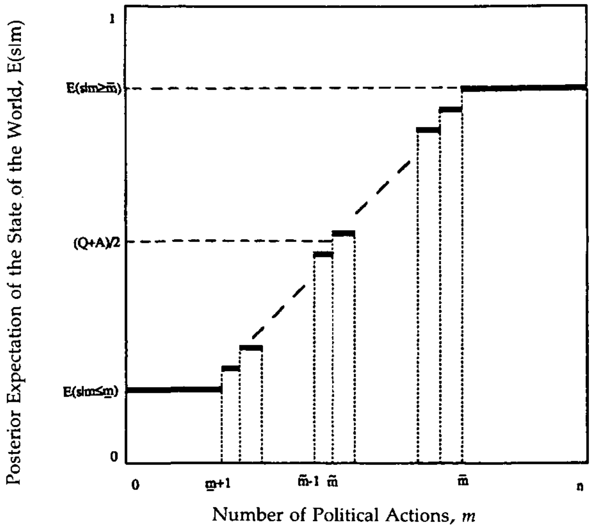
In equilibrium, the political leader knows that $\underline{m}$ extremists engage in manipulative political action. Thus, the observed number of political actions is discounted by $\underline{m}$. Similarly, the leader knows that the abstentions of $n - \overline{m}$ extremists are uninformative. It follows that the leader perfectly infers how many of the $\overline{m} - \underline{m}$ individuals whose bliss points lie in the interval $[\tilde{s}_1, \tilde{s}_0)$ are of type $\sigma = 1$, since these moderates take political action if and only if they are type $\sigma = 1$. The posterior probability that there are $m - \underline{m}$ individuals of type $\sigma = 1$ whose bliss points lie in the separating interval $[\tilde{s}_1, \tilde{s}_0)$ is equal to the binomial probability that $\overline{m} - \underline{m}$ Bernoulli trials result in $m - \underline{m}$ successes if the probability of a success is given by $s$, $b(m - \underline{m}; \overline{m} - \underline{m}, s)$. Using this information, the leader forms an inference on the state of the world $s$:

$$\beta(s|m) = b(m - \underline{m}; \overline{m} - \underline{m}, s)\beta(s)/ [\textstyle\int b(m - \underline{m}; \overline{m} - \underline{m}, s)\beta(s)\, ds]. \qquad (11)$$

The higher the number of individuals of type $\sigma = 1$ whose bliss points lie in the separating interval $[\tilde{s}_1, \tilde{s}_0)$, the higher is the posterior expectation of the state of the world (see Figure 3). If the estimated number of activist moderates exceeds the threshold level $\tilde{m}$, the leader infers that he or she would be better off shifting policy. If the number is strictly smaller, the leader strictly prefers to maintain the status quo, given the updated beliefs on the state of the world. The critical number of political actions $\tilde{m}$ is endogeneous, reflecting the leader's awareness of the individuals' incentives to inform or manipulate the leader's decision. In particular, the threshold number

FIGURE 3

**The Posterior Expectation of the State of the World as a Function of the Number of Political Actions**



*Note*:

| | |
|---|---|
| Q, A: | status quo and alternative, respectively |
| n: | total number of individuals |
| $\underline{m}$, $\bar{m}$: | minimum and maximum numbers of political actions, respectively |
| $\tilde{m}$: | critical number of political actions |

discounts the uninformative turnout of the anti-status-quo extremists.

In a partially revealing equilibrium, the information dispersed in the population is not fully revealed due to the distributional implications of the decision. Individuals whose preferences are extremely different from the political leader's expect to be better off if the leader is misinformed or remains uninformed. Some information that is pertinent to the policy decision is trapped in the following sense. In the aggregate, the population observes $n$ independent draws of the signal $\sigma$, but only the draws made by the $\bar{m} - \underline{m}$ individuals whose bliss points lie in the separating interval $[\bar{s}_1, \bar{s}_0)$ are revealed to the political leader. The observed number of political actions, $m$, may exceed the critical level $\tilde{m}$, although the state of the world $s$ lies below the critical number $(Q + A)/2$; and vice versa. In this case, the political leader "mistakenly" makes a decision that is disadvantageous for a majority of the population. The probability that a "mistaken" decision will be made is strictly higher in a partially revealing equilibrium than in the fully revealing one.

The group of activists contains individuals of both types $\sigma = 0$ and $\sigma = 1$. These two types have different information sets. Thus, my model implies that activists are heterogeneous in their estimates of the size of the protest movement and the associated probability of its success. Moreover, a higher proportion of activists have private information adverse to the status quo than do nonactivists. As a consequence, activists tend to expect a protest movement to be larger and more likely to succeed than do nonactivists.

The policy preferences and beliefs of the activists are not representative of the population as a whole. This lack of representativeness does not bias the leader's decision. The political leader takes into account that some individuals have disincentives to reveal their private information truthfully.[15] (In fact, the difference in beliefs about the state of the world drives the revelation of information through political action.)

In the presentation of the results, I have restricted attention to an equilibrium in which it is common knowledge that an individual who takes political action wishes to send the message "I am type $\sigma = 1$." Another equilibrium arises when the political action message is commonly believed to be "I am type $\sigma = 0$." In this case, political action is taken by pro-status-quo extremists and moderates of type $\sigma = 0$. Moreover, an uninformative zero political action equilibrium with self-fulfilling beliefs emerges if the individuals believe that no individual will turn out.

In the zero-cost case, an uninformative "babbling equilibrium" may also arise (Matthews 1989, 355). In this case, some or all individuals take political action with strictly positive probabilities regardless of their private information. It is interesting to note that this meaningless babble of protest is consistent with rational, utility-maximizing behavior.

The severity of the conflict of interest among the members of the society and the degree of uncertainty about the mapping of policies into policy outcomes determine whether the information dispersed in the population is fully aggregated. More formally, the degree of information aggregation depends on the relative size of the parameters $\bar{x}$ and $E[(s - 1/2)^2]$ (the latter parameter is normalized to 1/12). The analysis of the zero-cost case has demonstrated that the private information dispersed in the population may only be partially aggregated even if the cost of taking political action is zero. This result is due to binding incentive compatibility constraints arising from the heterogeneity of the population.[16]

**Proposition 2 (Strictly Positive But Not Prohibitive Cost)**

1. If the cost of taking political action is strictly positive but not prohibitive, then some individuals with moderate preferences take political action conditional on being of type $\sigma = 1$ and abstain otherwise. Regardless of their private information, individuals with extreme preferences in favor of the policy alternative take political action; individuals with extreme preferences in favor of the status quo abstain, as do some individuals with moderate preferences.
2. The political leader shifts policy if turnout exceeds a critical threshold.
3. The information dispersed in the population is only partially aggregated. Some information is trapped among some individuals with moderate preferences. If the population is heterogeneous,

the information held by individuals with extreme preferences is not revealed.

An individual who takes political action now derives some disutility from doing so, due to the cost of taking political action. He or she has an incentive to engage in costly political action only if the expected private benefits derived from a policy shift, multiplied by the expected probability that the action will be decisive in triggering a policy shift, exceed the private cost of taking political action. The individual's action is pivotal if and only if the number of political actions is exactly equal to $\tilde{m} - 1$ when the individual abstains.

Compared to the partially revealing zero-cost case, the political action cutpoints $\tilde{s}_1$ and $\tilde{s}_0$ shift (see Figure 2).[17] The underlying intuition is as follows. Individuals with identical information sets have identical estimates of the probability that their political actions will be pivotal in bringing about a policy shift. They also face identical costs of taking political action. The expected benefits that are derived from a policy shift increase in their bliss points. Thus, the individual who is indifferent toward taking costly political action when faced with a positive cost of taking action must have a lower bliss point than an individual with the same information set who is indifferent in the zero-cost case.

The political action cutpoints $\tilde{s}_0$ and $\tilde{s}_1$ implicitly solve the indifference conditions for two individuals of type $\sigma = 0$ and $\sigma = 1$ whose bliss points are exactly equal to $\tilde{s}_0$ and $\tilde{s}_1$, respectively:

$$\int \sum_{m=\underline{m}+1}^{\overline{m}} [\text{prob}(m \geq \tilde{m}) - \text{prob}(m - 1 \geq \tilde{m})] \cdot [(Q - s - \tilde{s}_0)^2 - (A - s - \tilde{s}_0)^2] \cdot b(m - \underline{m} - 1; \overline{m} - \underline{m} - 1, s)\beta(s|\sigma=0)\, ds = c, \qquad (12)$$

$$\int \sum_{m=\underline{m}+1}^{\overline{m}} [\text{prob}(m \geq \tilde{m}) - \text{prob}(m - 1 \geq \tilde{m})] \cdot [(Q - s - \tilde{s}_1)^2 - (A - s - \tilde{s}_1)^2] \cdot b(m - \underline{m} - 1; \overline{m} - \underline{m} - 1, s)\beta(s|\sigma=1)\, ds = c. \qquad (13)$$

Some comparative statics can be derived from these indifference conditions. The equilibrium probability that an individual will be pivotal increases in the expected private benefits from a policy shift, $E[(Q - s - x_i)^2 - (A - s - x_i)^2|\sigma]$, and decreases in the cost of taking political action, $c$. The action taken by one individual is pivotal with probability 0 only in the special and empirically irrelevant case of an infinite population. The probability that an individual action is decisive is strictly positive for a nonempty set of strictly positive cost parameters in a finite-sized society even if the society is very large; for any given size of the population and any given strictly positive but finite cost of taking political action, some individuals have incentives to take costly political action if the benefits expected from a policy shift are sufficiently large. This is the case if the policy alternatives are extremely polarized ($A - Q$ is large), if the population is very heterogeneous ($\bar{x}$ is large), or if the uncertainty about the state of the world is severe ($E[(s - 1/2)^2]$, here normalized to 1/12, is large].

Three assumptions are crucial for the result that turnout is strictly positive. First, the status quo and the policy alternative must be distinct policies. Otherwise, the individuals would be indifferent between the two policies; and no individual would have incentives to take action in order to bring about a policy shift. Second, the assumption that the realizations of the signal $\sigma$ are privately observed plays a crucial role. If they were publicly observed, then the number of political actions would not contain any new information, and no individual would have incentives to turn out. Third, the assumption that the signal $\sigma$ is correlated with the state of the world $s$ is important. If the number of individuals of either type, $\sigma = 0$ or $\sigma = 1$, were unrelated to the state of the world $s$, then the number of political actions would be uninformative, and no individual would have incentives to take action. The assumption that the individuals' private experiences are correlated with the state of the world ensures that one individual's experience, as expressed in that individual's action or abstention, has the potential to affect other individuals' and the political leader's policy preferences. The specific assumptions made about the correlation are not crucial. Instead, it is crucial that individuals with different private experiences have different expectations about the state of the world.

The political leader's response to political action takes into account the individuals' incentives to take political action. If these incentives are known to be weak, perhaps due to a high cost of taking political action, then a small number of political actions can be pivotal. The model implies that the success of a protest movement in triggering political change is a function of its actual size relative to its ex ante expected size. The critical number of political actions that triggers a policy shift is a function of the parameters that affect the individuals' incentives to engage in political action: the cost of taking political action, the polarization of the policy alternatives, the heterogeneity of the population, and the degree of uncertainty about the state of the world.

As in the zero-cost case, some moderates take informative political action. However, there now exists a subset of moderates who do not engage in political action even if they expect to be better off if the political leader shifted policy. These rationally apathetic moderates are close to indifferent between the status quo and the policy alternative and do not have an incentive to pay the private cost of taking political action. They free-ride on the informational efforts of the activist moderates. Thus, even if the population is fairly homogeneous, some information will be trapped in rationally apathetic pockets of the society (see Figure 2).[18]

If the population is very heterogeneous, the private information held by individuals with extreme preferences is not revealed due to the distributional effects of information revelation (see Figure 2). In their futile attempt to manipulate the political leader's decision, anti-status-quo extremists are trapped into taking costly but uninformative political action. They know that the political leader discounts the size of the protest movement to take into account that their political actions are uninformative. Given the leader's updating rule, they are "forced" to take political action to prevent an unacceptable reduction in the probability that the leader will shift policy.

Pro-status-quo extremists abstain from political action. But their abstentions are *not* motivated by the desire to free-ride and avoid the cost of taking political action. They do not have incentives to reveal their private information because they prefer the political leader to make an uninformed decision.

As in the zero-cost case, other equilibria may arise in which political action is taken by individuals who favor the status quo or in which no individual turns out.

This analysis demonstrates that both strictly positive costs of taking political action and conflicts of interest prevent the full revelation of the private information dispersed in the population. Nevertheless, if the cost of taking political action is strictly positive but not prohibitive, some individuals may have incentives to take informative political action.

**Proposition 3 (Prohibitive Cost)**

1. If the cost of taking action is prohibitive, then all individuals abstain from taking political action.
2. The political leader maintains the status quo if the status quo is ex ante expected to be advantageous for a majority of the population and shifts policy otherwise.
3. None of the information dispersed in the population is revealed.

For prohibitive costs of taking action, no individual turns out:

$$\pi = 0. \tag{14}$$

As a consequence, the political leader's decision is based on his or her priors

$$\beta(s|m) = \beta(s), \tag{15}$$

and the leader's decision rule is given by

$$\rho = 0 \tag{16}$$

It is interesting to note that if the status quo is ex ante expected to be advantageous for a majority, then uninformed decision-making is biased toward the status quo.

## EXTENSIONS OF THE FRAMEWORK

The framework developed here is a first attempt to synthesize signaling and participation games. The basic model can be extended to examine other normative and positive claims about political action that cannot be dealt with in the compass of a single paper.

One variation on the model analyzes preelection political action (Lohmann 1992b, n.d.). The political action mechanism aggregates some of the individuals' private information prior to a vote at a cost privately incurred by the activists. This mechanism coexists with the voting mechanism that costlessly aggregates some of the dispersed information via the vote.

By informing voting decisions, political action has the potential to increase the likelihood that the full-information voting outcome is achieved. Perhaps surprisingly, however, preelection communication may be counterproductive. Given that some information is aggregated by the vote, the marginal contribution of the political action mechanism may consist in adding noise. In some cases, the voting mechanism is ex ante more likely to implement the full-information voting outcome in the absence of preelection political action.

Lohmann (1992a) allows for the possibility that individuals on both sides of an issue take counteracting political action. The individuals on one side generally have stronger incentives to become active than do those on the other side. A political leader rationally takes into account differential participation incentives when extracting information from the political action turnout on each side. As a consequence, asymmetric participation incentives do *not* cause a systematic policy bias in favor of the more active side.

This result contrasts with the commonly held view that political decisions will tend to be biased in favor of interests that are more successful in overcoming the free-rider problem of political participation. Such biases may, of course, emerge once more institutional structure is imposed on the problem or if political pressures impact on political decisions in ways that are not purely informational.

I elsewhere model a sequence of protest activities as an informational cascade (Lohmann 1992c). The seemingly haphazard dynamics of a protest movement are endogeneously driven by the revelation of information through changes in mass turnout over time. Turnout tends to vacillate for a while before exploding or evaporating. In some cases, a small number of political actions may have a large impact on public opinion, while a huge turnout may be followed by the sudden collapse of the protest movement.

Other possible extensions of my work would allow individuals to engage in costly information-gathering efforts prior to the decision to take political action or would have individuals pay a fee to join an interest group that represents its members in taking political action. In addition, people might choose between low-cost and high-cost forms of political action. Empirically, this extension might reflect the choice between legal and illegal, nonviolent and violent, means of political protest. Similarly, the cost of lobbying activities and the size of campaign contributions might be informative for voters or policymakers.[19]

Political leaders have access to a variety of sources that provide information about the preferences of the populace. For example, the information revealed through public opinion polls might be a partial substitute for the information that is revealed through political action. However, polls are typically conducted in a centralized way and reflect the priors of their organizers as to which questions are of importance in the public debate. In contrast, the individuals' decisions to engage in political action are made in a decentralized way. In a world that changes over time in unforeseen ways, political action has the potential to make "socially invisible" issues politically salient. For example, a small minority of individuals might become aware of unforeseen hazards associated with some previously uncontroversial social activity. By taking political action, they can make the public and its political leaders aware of these hazards and thereby trigger a policy response.

The framework developed here is based on the assumption that a political leader makes a decision with regard to one policy issue. However, political leaders typically deal with a multiplicity of issues. According to Converse's (1964) concept of constraint, the issue space might be multidimensional, but individual belief systems are constrained in the sense that the knowledge about an individual's policy position on one issue can be used to predict the individual's policy position on another. As a consequence, political leaders might position themselves on a liberal–conservative dimension. Their positions on this dimension might summarize in a simple way how they would vote on a multiplicity of issues (Enelow and Hinich 1990; Poole and Rosenthal 1991). It would be of interest to examine whether and how political action changes the mapping of policy positions in the multidimensional issue space into positions on the one-dimensional liberal–conservative space. By making new issues politically salient or changing the relative political weights of various issues, political action may play a role in triggering political realignments (Sundquist 1983).

## APPENDIX

I shall show that the equilibrium strategies and beliefs presented in the text fulfill conditions E1–E4. As noted in the text, the equilibrium is unique only for the case of prohibitive costs. This proof shows that a best response is unique, given the best responses of the other players and the beliefs of all players. Similarly, the beliefs held by one player are shown to be unique, given the best responses of all players and the beliefs of the other players. For example, in the case of strictly positive but not prohibitive costs, a partially revealing equilibrium is characterized by a set of cutpoints, $\bar{s}_0$, $\bar{s}_1$ and $\bar{m}$, that implicitly solve equations 10, 12, and 13. This system of equations may have multiple solutions; but any one cutpoint is uniquely determined, once the other two cutpoints are fixed.

### Political Action Stage

*Claim.* The beliefs of an individual of type $(j, \sigma)$, $\beta(s|j, \sigma) = \beta(s|\sigma)$ in equation 3, are the unique Bayesian posterior on the density of the state of the world at the political action stage, given the individual's private information and common knowledge priors on Nature's actions.

*Proof.* The claim immediately follows from the assumed correlation between the binary signal $\sigma$ and the state of the world $s$ (see equations 1 and 2) and straightforward application of Bayes' Rule.

*Claim.* Given $\{\pi(i, \sigma)\}_{-j}$, $\{\beta(s|\sigma)\}$, $\rho(m)$, and $\beta(s|m)$, the unique best response of an individual of type $(j, \sigma)$ in an informative equilibrium is given by $\pi(j, \sigma)$ in equation 6; $\pi(j, \sigma)$ in equation 13 is one of multiple best responses in the uninformative equilibria of the zero-cost case and the unique best response in the uninformative equilibrium for strictly positive costs.

*Proof.* In the zero-cost case, the expected loss of an individual of type $(j, \sigma)$ conditional on his or her information set at the political action stage, $I(\sigma)$, is given by

$$E(L_j|\sigma) = \int [\text{prob}(\rho = 0)(Q - s - x_j)^2 + \text{prob}(\rho = 1)(A - s - x_j)^2]\beta(s|\sigma)\, ds. \quad \textbf{(A-1)}$$

In the fully revealing equilibrium in the zero-cost case, equation A-1 is equivalent to

$$E(L_j|\sigma) = \int \sum_{m=1}^{n} \{\text{prob}[\rho(m - 1 + \pi) = 0](Q - s - x_j)^2 + \text{prob}[\rho(m - 1 + \pi) = 1](A - s - x_j)^2\} \cdot b(m - 1; n - 1, s)\beta(s|\sigma)\, ds. \quad \textbf{(A-2)}$$

Given the political leader's cutpoint decision rule, equation A-2 is equivalent to

$$E(L_j|\sigma) = \int \sum_{m=1}^{n} [\text{prob}(m - 1 + \pi < \bar{m})(Q - s - x_j)^2 + \text{prob}(m - 1 + \pi \geq \bar{m})(A - s - x_j)^2] \cdot b(m - 1; n - 1, s)\beta(s|\sigma)\, ds.$$

Individual j takes political action if

$$\int \sum_{m=1}^{n} [\text{prob}(m \geq \tilde{m}) - \text{prob}(m-1 \geq \tilde{m})][(Q - \mathsf{s} - x_j)^2 - (A - \mathsf{s} - x_j)^2] b(m-1; n-1, \mathsf{s}) \beta(\mathsf{s}|\sigma)\, d\mathsf{s} > 0. \quad \textbf{(A-3)}$$

Individual $j$ abstains if the inequality in equation A-3 is negative and is indifferent if equation A-3 holds with equality. Given the political leader's decision rule, it holds that

$$\int \sum_{m=1}^{n} [\text{prob}(m \geq \tilde{m}) - \text{prob}(m-1 \geq \tilde{m})] \cdot b(m-1; n-1, \mathsf{s}) \beta(\mathsf{s}|\sigma)\, d\mathsf{s} > 0. \quad \textbf{(A-4)}$$

The inequality in equation A-4 implies that the expression in equation A-3 is strictly positive for $x_j > (Q + A)/2 - E(s|\sigma)$, strictly negative for $x_j < (Q + A)/2 - E(s|\sigma)$, and holds with equality for $x_j = (Q + A)/2 - E(s|\sigma)$. It follows that the cutpoint rule given in equation 6 is the unique best response for all individuals at the political action stage, except for the knife-edge indifferent individuals of type $\sigma = 0$ and $\sigma = 1$ whose bliss points are exactly equal to $(Q + A)/2 - E(s|\sigma = 0)$ and $(Q + A)/2 - E(s|\sigma = 1)$, respectively.

In the partially revealing equilibrium in the zero-cost case, equation A-3 is modified to

$$\int \sum_{m=\underline{m}+1}^{\overline{m}} [\text{prob}(m \geq \tilde{m}) - \text{prob}(m-1 \geq \tilde{m})] \cdot [(Q - \mathsf{s} - x_j)^2 - (A - \mathsf{s} - x_j)^2] \cdot b(m - \underline{m} - 1; \overline{m} - \underline{m} - 1, \mathsf{s}) \beta(\mathsf{s}|\sigma)\, d\mathsf{s} > 0, \quad \textbf{(A-5)}$$

but the above arguments continue to hold.

In a partially revealing equilibrium in the case of strictly positive but not prohibitive costs, equation A-1 is modified to include the expected cost of taking action:

$$E(L_j|\sigma) = \int [\text{prob}(\rho = 0)(Q - \mathsf{s} - x_j)^2 + \text{prob}(\rho = 1)(A - \mathsf{s} - x_j)^2] \beta(\mathsf{s}|\sigma)\, d\mathsf{s} + \pi(j, \sigma)c.$$

Now individual $j$ takes action if

$$\int \sum_{m=\underline{m}+1}^{\overline{m}} [\text{prob}(m \geq \tilde{m}) - \text{prob}(m-1 \geq \tilde{m})] [(Q - \mathsf{s} - x_j)^2 - (A - \mathsf{s} - x_j)^2] \cdot b(m - \underline{m} - 1; \overline{m} - \underline{m} - 1, \mathsf{s}) \beta(\mathsf{s}|\sigma)\, d\mathsf{s} > c. \quad \textbf{(A-6)}$$

Individual $j$ abstains if the inequality in equation A-6 is negative and is indifferent if equation A-6 holds with equality. By definition of the cutpoints $\tilde{s}_0$ and $\tilde{s}_1$ in equations 12 and 13, the inequality in equation A-6 is strictly positive for all individuals of type $\sigma = 0$ whose bliss points lie strictly above $\tilde{s}_0$ and for all individuals of type $\sigma = 1$ whose bliss points lie strictly above $\tilde{s}_1$. The inequality is strictly negative for all individuals of type $\sigma = 0$ whose bliss points lie strictly below $\tilde{s}_0$ and for all individuals of type $\sigma = 1$ whose bliss points lie strictly below $\tilde{s}_1$. It follows that the cutpoint rule given in equation 6 is the unique best response for all individuals at the political action stage, except for the knife-edge indifferent individuals of type $\sigma = 0$ and $\sigma = 1$ whose bliss points are exactly equal to $\tilde{s}_0$ and $\tilde{s}_1$, respectively.

In the uninformative equilibria, the political leader makes a decision based on his prior information. In the zero-cost case, the individuals are indifferent toward taking action, so that both zero political action and babbling equilibria can be supported. In the case of strictly positive costs, the individuals strictly prefer to abstain from taking action. Given the restriction imposed on the political leader's off-the-equilibrium-path beliefs, the individuals cannot affect the outcome by deviating from the equilibrium prescription and thus have no incentive to do so.

### Policy Decision Stage

*Claim.* Given $\{\pi(i,\sigma)\}$, $\{\beta(s|\sigma)\}$, $\rho(m)$, and the common knowledge priors on Nature's actions, the political leader forms the unique Bayesian posterior on the density of the state of the world at the policy decision stage, $\beta(s|m)$, given in equation 7 for the fully revealing equilibrium, in equation 11 for a partially revealing equilibrium, and in equation 15 for the uninformative equilibria.

*Proof.* In the fully revealing equilibrium of the zero cost case, all individuals of type $\sigma = 1$ take political action, while all other abstain. The political leader forms a perfect inference on the number of individuals of type $\sigma = 1$, $m$. Given the correlation between the individuals' private information $\sigma$ and the state of the world $s$ (see equations 1 and 2), the probability that $m$ individuals out of a population of $n$ individuals are of type $\sigma = 1$ if the state of the world is given by $s$ is equal to the binomial probability that $n$ Bernoulli trials result in $m$ successes if the probability of a success is given by $s$, $b(m; n, s)$. Straightforward application of Bayes' Rule leads to the posterior on the state of the world $s$ given in equation 7.

In a partially revealing equilibrium, the political leader knows that informative political action is taken only by the $\overline{m} - \underline{m}$ individuals whose bliss points lie in the separating interval $[\tilde{s}_1, \tilde{s}_0)$. Based on the political action turnout $m$, the leader forms the perfect inference that there are $m - \underline{m}$ individuals of type $\sigma = 1$ whose bliss points lie in the separating interval $[\tilde{s}_1, \tilde{s}_0)$. The posterior probability that $m - \underline{m}$ out of $\overline{m} - \underline{m}$ individuals are of type $\sigma = 1$ if the state of the world is given by $s$ is equal to the binomial probability that $\overline{m} - \underline{m}$ Bernoulli trials result in $m - \underline{m}$ successes if the probability of a success is given by $s$, $b(m - \underline{m}; \overline{m} - \underline{m}, s)$. Straightforward application of Bayes' Rule leads to the posterior on the state of the world given in equation 11.

In the *uninformative equilibria*, the political action turnout is not informative. Thus the principal's decision is based on his or her priors, as specified in equation 15. This is also the case if any individual deviates from his or her equilibrium political action strategy, given the restriction placed on the political leader's off-the-equilibrium-path beliefs in (E4).

*Claim.* Given $\{\pi(i, \sigma)\}$, $\{\beta(s|\sigma)\}$, and $\beta(s|m)$, the unique best response for the political leader at the policy decision stage is given by $\rho(m)$ in equation 10 for an informative equilibrium and in equation 15 for the uninformative equilibria.

*Proof.* The political leader's expected loss at the policy decision stage, conditional on his information set $I(m)$, is given by

$$E(L_0|m) = \int \{\text{prob}[\rho(m) = 0](Q - s)^2 + \text{prob}[\rho(m) = 1] \cdot (A - s)^2\}\beta(s|m)\, ds$$

$$= \int \{[1 - \text{prob}[\rho(m) = 1]](Q - s)^2 + \text{prob}[\rho(m) = 1](A - s)^2\}\beta(s|m)\, ds.$$

The derivative of $E(L_0|m)$ with respect to prob $[\rho(m) = 1]$ is equal to zero for $E(s|m) = (Q + A)/2$, strictly positive for $E(s|m) < (Q + A)/2$ and strictly negative for $E(s|m) > (Q + A)/2$. It follows that the political leader uniquely minimizes $E(L_0|m)$ by setting prob$[\rho(m) = 1]$ equal to one if $E(s|m) > (Q + A)/2$, and equal to zero if $E(s|m) < (Q + A)/2$. The principal is indifferent if $E(s|m) = (Q + A)/2$. This case is knife-edge since $m$ is constrained to be a natural number.

In an informative equilibrium, the expectation $E(s|m)$ is increasing in $m$, given the posteriors 7 and 11. Equations 8 and 9 define the natural number $\tilde{m}$ to be the threshold level of $m$ such that if $m \geq \tilde{m}$, then $E(s|m) \geq (Q + A)/2$, and if $m < \tilde{m}$, then $E(s|m) < (Q + A)/2$. It follows that the cutpoint rule given in equation 10 is the unique best response for the political leader.

In the uninformative equilibria, the political leader's decision is based on his priors. Given that $E(s) = 1/2 < (Q + A)/2$, it immediately follows that the decision rule given in equation 16 is the unique best response. The political leader is indifferent only in the knife-edge case of $E(s) = 1/2 = (Q + A)/2$.

QED.

## Notes

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1. The elitist theory of democracy is critically reviewed in Walker 1966.

2. The model is easily modified to have the leader represent a minority of the population, with no qualitative change in the results.

3. The model is easily modified to allow the leader to have some imperfect private information, with no qualitative change in the results.

4. I discuss an extension of the model in which proponents of the status quo may choose to take counteracting political action in Lohmann 1992a.

5. Baron's (1988) analysis of political action is based on an exogeneously given functional relationship between the probability that a regulator reneges on a commitment made to a regulated firm and the amount of political action taken by the firm and a consumer interest group. DeNardo proposes that a regime collapses if the percentage of the population mobilized against the regime exceeds an exogeneously given "revolutionary threshold" (1985, 101).

6. The case of n even is an easy extension that does not qualitatively affect the results.

7. One possible extension of the model would allow for incomplete information about the voters' preferences over policy outcomes. This extension would complicate the equations by adding another layer of uncertainty without qualitatively changing the results.

8. The assumption of uniform priors is not crucial for the analysis.

9. The specific assumptions about the location of the two policies are not crucial for the analysis; the same holds for the assumptions about the distributions of the states of the world and the individuals' bliss points. Policy decisions are often binary, focusing on the choice between the status quo and a distinct policy alternative. I examine the case where political action affects the probability that the status quo is overturned in favor of the policy alternative. One interesting extension of the model would allow political action to be directed at influencing the location of the policy alternative.

10. The models I develop here and in Lohmann 1992b could be synthesized to analyze the situation in which two office-motivated candidates, an incumbent and a challenger, compete in a majority-rule election. Based on the information revealed through political action, the two candidates form an inference about the policy preferences of a majority and choose their policy positions. Each of the two candidates is constrained to choose either the status quo ($\omega = 0$) or the policy alternative ($\omega = 1$) and is committed to implement the chosen policy if elected. Next, each voter chooses whether to vote in favor of the status quo candidate ($\nu = 0$) or for the candidate who is committed to the policy alternative ($\nu = 1$). I assume that the incumbent has an advantage: the individuals vote in favor of the incumbent if they are indifferent between the two candidates (which happens if the candidates choose identical positions). The incumbent and the challenger have identical utility functions:

$$U_j = \begin{cases} 1 & \text{if } \sum_{i=1}^{n} \nu_i \geq (n+1)/2 \text{ and } \omega_j = 1 \\ & \text{or if } \sum_{i=1}^{n} \nu_i < (n+1)/2 \text{ and } \omega_j = 0 \\ 0 & \text{otherwise,} \end{cases}$$

where the voters are indexed by subscript i, the candidates, by subscript j. One equilibrium of this model would have the incumbent choose the policy that is expected to get a majority in the election, while the challenger would commit to the other policy. Interestingly, individuals would not have incentives to take political action in order to influence the candidates' position taking, because they know that each position will be represented by one candidate in equilibrium. Instead, individuals would take action to inform other voters and thereby influence the election outcome.

The existence of the incumbency advantage is crucial for the "polarized" positioning of the candidates. If no candidate has an advantage, then the candidates would both choose the position that is expected to get a majority in the election. In this case, the voters would be indifferent between the two candidates; and no individual would have incentives to take political action to influence the vote. Instead, individuals would take action to influence the candidates' position taking.

11. For simplicity, I assume that the probability of observing an event of type $\sigma = 0$ or $\sigma = 1$ is perfectly correlated with the state of the world $s$. The assumption of perfect correlation is not crucial for the results.

12. In Lohmann 1992a, I discuss an extension of the model in which individuals can choose between sending one (or both) of the messages "I am type $\sigma = 0$" and "I am type $\sigma = 1$" and abstaining from sending a message.

13. The model is easily extended to have the political leader privately observe one or more draws of the signal $\sigma$, with no qualitative change in the results. Moreover, the model could be generalized to allow for an additional source of uncertainty about which the political leader has some private "expert" information.

14. This refinement has the advantage of being both simple and plausible. The refinement is not identical to, or implied by, any of the standard refinements in the literature (e.g., Banks and Sobel 1987; Cho and Kreps 1987). One motivation for my refinement is that individuals might make mistakes (Selten 1975); that is, some individuals might not follow their equilibrium prescription to take political action or abstain. In principle, the model could be modified to allow for mistakes. However, I conjecture that this extension would add another layer of uncertainty without qualitatively changing the results. For simplicity, I prefer to refine the equilibrium concept by imposing an ad hoc restriction on the political leader's out-of-equilibrium beliefs. The motivation for refinements of sequential equilibrium that place restrictions on players' out-of-equilibrium beliefs is generally ad hoc (Kreps 1989).

15. This result is related to Milgrom and Roberts's (1986) finding that information contributed by interested parties is discounted. In a different setting, Calvert (1985) shows that a superior may be more responsive if a subordinate who is known to be biased *against* an alternative recommends that this alternative be chosen.

16. This result is related to Crawford and Sobel's (1985) result that "cheap talk" is less informative when the conflict of interest between the communicating parties is severe.

17. An equilibrium with a strictly positive expected number of political actions is necessarily characterized by $\bar{s}_1 < \bar{s}_0$ if it is commonly understood that an individual who takes political action wishes to convey the message "I am type $\sigma = 1$." Otherwise, the probability that the political leader shifts policy would be decreasing in the number of political actions, m. In this case, individuals for whom the equilibrium prescription is to take political action because they expect to benefit from a policy shift would have incentives to deviate from their equilibrium strategy.

18. This result is consistent with the analysis of individual incentives to contribute to the provision of a public good (in this case, information) at a private cost (Palfrey and Rosenthal 1984).

19. Recent signaling models of interest group activity and lobbying are provided in Ainsworth and Sened 1991; Ball 1991; Rasmusen 1991; and Austen-Smith and Wright 1990.

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# SOCIAL CONSTRUCTION OF TARGET POPULATIONS: IMPLICATIONS FOR POLITICS AND POLICY

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*We argue that the social construction of target populations is an important, albeit overlooked, political phenomenon that should take its place in the study of public policy by political scientists. The theory contends that social constructions influence the policy agenda and the selection of policy tools, as well as the rationales that legitimate policy choices. Constructions become embedded in policy as messages that are absorbed by citizens and affect their orientations and participation. The theory is important because it helps explain why some groups are advantaged more than others independently of traditional notions of political power and how policy designs reinforce or alter such advantages. An understanding of social constructions of target populations augments conventional hypotheses about the dynamics of policy change, the determination of beneficiaries and losers, the reasons for differing levels and types of participation among target groups, and the role of policy in democracy.*

Contemporary political scientists consider many variables to be significant political phenomena that previously were viewed either as irrelevant or as the proper domain of another discipline. The importance of gender in understanding political behavior and the role of money and media in politics are examples. Although the question of who benefits or loses from policy has long been interesting to political science, most other dimensions of policy designs have been considered the purview of economists, lawyers, and other specialists. With the emergence of public policy as a major subfield of political science, however, attention has turned to new aspects of the policy process, such as agenda setting, formulation, implementation, and consequences, (Arnold 1990; Ingram and Schneider 1991; Kingdon 1984; Lipsky and Smith 1989; Mazmanian and Sabatier 1983; Rose 1991; Smith and Stone 1988; Pressman and Wildavsky 1973) as well as additional elements of policy design, such as goals, tools, rules and target populations (Ingram and Schneider 1992; Linder and Peters 1985; Ostrom 1990; Schneider and Ingram 1990a, 1990b; Stone 1988). We argue that the social construction of target populations is an important, albeit overlooked, political phenomenon that should take its place in the study of public policy by political scientists.

The social construction of target populations refers to the cultural characterizations or popular images of the persons or groups whose behavior and well-being are affected by public policy. These characterizations are normative and evaluative, portraying groups in positive or negative terms through symbolic language, metaphors, and stories (Edelman 1964, 1988). A great deal has been written (mostly by sociologists) about social constructions of social problems (Best 1989; Spector and Kitsuse 1987). The more specific topic of social construction of target populations is important to political science because it contributes to studies of agenda setting, legislative behavior, and policy formulation and design, as well as to studies of citizen orientation, conception of citizenship, and style of participation.

Our theory contends that the social construction of target populations has a powerful influence on public officials and shapes both the policy agenda and the actual design of policy. There are strong pressures for public officials to provide beneficial policy to powerful, positively constructed target populations and to devise punitive, punishment-oriented policy for negatively constructed groups. Social constructions become embedded in policy as messages that are absorbed by citizens and affect their orientations and participation patterns. Policy sends messages about what government is supposed to do, which citizens are deserving (and which not), and what kinds of attitudes and participatory patterns are appropriate in a democratic society. Different target populations, however, receive quite different messages. Policies that have detrimental impacts on, or are ineffective in solving important problems for, certain types of target populations may not produce citizen participation directed toward policy change because the messages received by these target populations encourage withdrawal or passivity. Other target populations, however, receive messages that encourage them to combat policies detrimental to them through various avenues of political participation.

The theory is important because it helps explain why some groups are advantaged more than others independently of traditional notions of political power and how policy designs can reinforce or alter such advantages. Further, the theory resolves some long-standing puzzles political scientists have encountered in attempting to answer Lasswell's question, "Who gets what, when, and how?" (Lasswell 1936). The theory returns public policy to center-stage in the study of politics, offering an alternative that goes beyond both the pluralist and the microeconomic perspectives.

## THE SOCIAL CONSTRUCTION OF TARGET POPULATIONS

A theory that connects social constructions of target populations to other political phenomena needs definitions of target populations and of social constructions, an explanation of how social constructions influence public officials in choosing the agendas and designs of policy, and an explanation of how policy agendas and designs influence the political orientations and participation patterns of target populations.

### Conceptualizing Targets and Constructions

Target population is a concept from the policy design literature that directs attention to the fact that policy is purposeful and attempts to achieve goals by changing people's behavior (see our earlier work, Ingram and Schneider 1991). Policy sets forth problems to be solved or goals to be achieved and identifies the people whose behavior is linked to the achievement of desired ends. Behavioral change is sought by enabling or coercing people to do things they would not have done otherwise. By specifying eligibility criteria, policy creates the boundaries of target populations. Such groups may or may not have a value-based cultural image, however. Therefore, they may or may not carry out any social construction.

The social construction of a target population refers to (1) the recognition of the shared characteristics that distinguish a target population as socially meaningful, and (2) the attribution of specific, valence-oriented values, symbols, and images to the characteristics. Social constructions are stereotypes about particular groups of people that have been created by politics, culture, socialization, history, the media, literature, religion, and the like. Positive constructions include images such as "deserving," "intelligent," "honest," "public-spirited," and so forth. Negative constructions include images such as "undeserving," "stupid," "dishonest," and "selfish." There are a wide variety of evaluative dimensions, both positive and negative, that can be used to portray groups.

Social constructions are often conflicting and subject to contention. Policy directed at persons whose income falls below the official poverty level identifies a specific set of persons. The social constructions could portray them as disadvantaged people whose poverty is not their fault or as lazy persons who are benefitting from other peoples' hard work. On the other hand, not all target populations even have a well-defined social construction. Motor vehicle policies identify automobile drivers as a target population; but these persons have no particular social construction, at this time. Policies directed at drunk drivers or teenage drivers, however, have identified a subset that carries a negative valence.

The actual social constructions of target groups, as well as how widely shared the constructions are, are matters for empirical analysis. Social constructions of target populations are measurable, empirical, phenomena. Data can be generated by the study of texts, such as legislative histories, statutes, guidelines, speeches, media coverage, and analysis of the symbols contained therein. Social constructions also can be ascertained from interviews or surveys of policymakers, media representatives, members of the general public, and persons within the target group itself.

One of the major contentions of some social constructionists (sometimes called strict-constructionists) is that there is no objective reality but only the construction itself (Spector and Kitsuse 1987, J. Schneider 1985). Those who make this argument contend that research should focus on the constructions, not on the reasons the constructions have arisen or how constructions differ from objective reality. The point of view adopted here, however, is more like that expressed by Edelman (1988) and Collins (1989). Target populations are assumed to have boundaries that are empirically verifiable (indeed, policies create these empirical boundaries) and to exist within objective conditions even though those conditions are subject to multiple evaluations. One of the important issues for analysis is to understand how social constructions emerge from objective conditions and how each changes.

### Social Constructions and Elected Officials

Research has uncovered a number of important motivations for elected officials (Arnold 1990; Kelman 1987; Kingdon 1984). Two of the most important are to produce public policies that will assist in their reelection and that will be effective in addressing widely acknowledged public problems. Social constructions are relevant for both of these considerations.

Social constructions become part of the reelection calculus when public officials anticipate the reaction of the target population itself to the policy and also anticipate the reaction of others to whether the target group *should* be the beneficiary (or loser) for a particular policy proposal (Wilson 1986). Thus, the electoral implication of a policy proposal depends partly on the power of the target population itself (construed as votes, wealth, and propensity of the group to mobilize for action) but also on the extent to which others will approve or disapprove of the policy's being directed toward a particular target.

The convergence of power and social constructions creates four types of target populations, as displayed in Figure 1. Advantaged groups are perceived to be both powerful and positively constructed, such as the elderly and business. Contenders, such as unions and the rich, are powerful but negatively constructed, usually as undeserving. Dependents might

FIGURE 1

**Social Constructions and Political Power: Types of Target Populations**

| | | Constructions | |
|---|---|---|---|
| | | Positive | Negative |
| Power | Strong | ***Advantaged***<br>The elderly<br>Business<br>Veterans<br>Scientists | ***Contenders***<br>The rich<br>Big unions<br>Minorities<br>Cultural elites<br>Moral majority |
| | Weak | ***Dependents***<br>Children<br>Mothers<br>Disabled | ***Deviants***<br>Criminals<br>Drug addicts<br>Communists<br>Flag burners<br>Gangs |

include children or mothers and are considered to be politically weak, but they carry generally positive constructions. Deviants, such as criminals, are in the worst situation, since they are both weak and negatively constructed. Public officials find it to their advantage to provide beneficial policy to the advantaged groups who are both powerful and positively constructed as "deserving" because not only will the group itself respond favorably but others will approve of the beneficial policy's being conferred on deserving people. Similarly, public officials commonly inflict punishment on negatively constructed groups who have little or no power, because they need fear no electoral retaliation from the group itself and the general public approves of punishment for groups that it has constructed negatively. Figure 1 shows other examples of how a hypothetical elected official might array a variety of target populations within these dimensions.

Some social constructions may remain constant over a long period of time, as have the prevailing constructions of criminals or communists; but others are subject to continual debate and manipulation. For instance, persons with AIDS are constructed by some as deviants, little better than criminals who are being punished through disease for their sins. The identification of children, hemophiliacs, heterosexuals, and Magic Johnson as victims, however, has made possible a different construction. Public officials realize that target groups can be identified and described so as to influence the social construction. Hence, a great deal of the political maneuvering in the establishment of policy agendas and in the design of policy pertains to the specification of the target populations and the type of image that can be created for them.

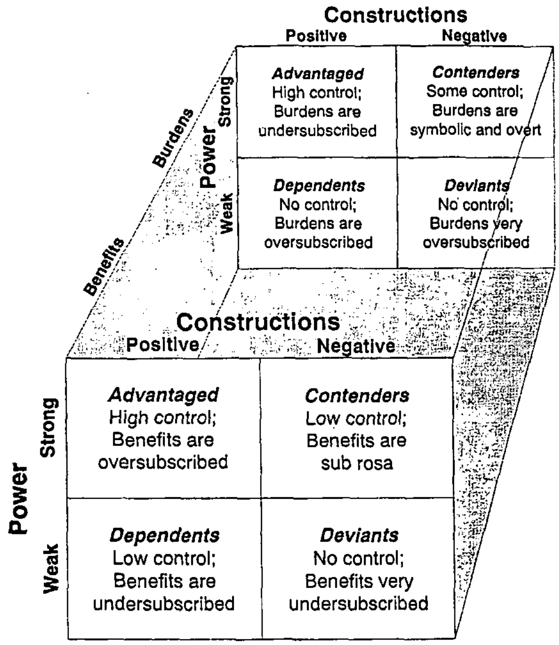
Social constructions may become so widely shared that they are extremely difficult to refute even by the small number of persons who might disagree with them. Other constructions, however, are in contention. Officials develop maps of target populations based on both the stereotypes they themselves hold and those they believe to prevail among that segment of the public likely to become important to them. Competing officials champion different constructions of the same groups. Some view minorities as oppressed populations and argue for policies appropriate to dependent people, whereas others portray minorities as powerful special interests and not deserving of government aid. Political debates may lead elected officials to make finer and finer distinctions, thereby subdividing a particular group into those who are deserving and those who are not. Immigration policy, for example, distinguishes among illegal aliens, refugees, migrant workers, those seeking asylum, and highly skilled workers who receive waivers. There has been no research on the social constructions of target populations from the perspective of elected officials; thus, there is no way to speculate on how Figure 1 actually should be drawn and how much agreement there would be about the placement of various groups.

Public officials are sensitive not only to power and social construction but also to pressure from the public and from professionals to produce effective public policies (Arnold 1990; Kelman 1987; Quade 1982). Public officials must explain and justify their policy positions to the electorate by articulating a vision of the public interest and then showing how a proposed policy is logically connected to these widely shared public values (Arnold 1990; Habermas 1975; Offe 1985). They need to have a believable causal logic connecting the various aspects of the policy design to desired outcomes.

Social constructions of target populations become important in the policy effectiveness calculus because elected officials have to pay attention to the logical connection between the target groups and the goals that might be achieved. Elected officials may emphasize some goals rather than others because target populations that they wish to benefit or burden have credible linkages to the goals (Edelman 1988; Kingdon 1984). On the other hand, elected officials are able to construct several different policy logics for almost any problem they wish to solve. For example, most would agree that reduction in the infant mortality rate in the United States is a worthy goal. However, to achieve this, the United States could provide direct health care benefits to high-risk pregnant women, it could mandate reductions in carcinogens that presumably increase risk, or it could criminalize drug and alcohol use by pregnant women. All of these could be justified as contributing to a reduced infant mortality rate; but they have very different implications for target populations, especially pregnant women who could either be the beneficiary of the policy or could bear exceptional costs because of it. Economic vitality is another example of a widely shared public goal for which a credible case could be made for policies that serve

FIGURE 2

Variations in How Policy Treats Target Populations: Allocation of Benefits and Burdens


Constructions
Positive
Negative
Burdens
Power
Strong
Weak
Advantaged
High control;
Burdens are
undersubscribed
Contenders
Some control;
Burdens are
symbolic and overt
Dependents
No control;
Burdens are
oversubscribed
Deviants
No control;
Burdens very
oversubscribed
Benefits
Constructions
Positive
Negative
Power
Strong
Weak
Advantaged
High control;
Benefits are
oversubscribed
Contenders
Low control;
Benefits are
sub rosa
Dependents
Low control;
Benefits are
undersubscribed
Deviants
No control;
Benefits very
undersubscribed

*Note: Benefits are shown on the front part of the box to each type of target; burdens are shown at the back of the box.*

widely divergent interests. Some policy options would give direct benefits to jobless or low-income persons, whereas other options would redistribute wealth to the poor, thereby increasing demand for products. Others would offer tax breaks, loans, or outright grants to the owners of businesses to increase their competitive position or to entice them to move into a location (or to retain those who are threatening to leave). In almost any policy area there are multiple logics that involve different target populations and/or different roles for target groups. Thus, even when public officials are pursuing widely held public interest goals, they are commonly able to provide benefits to powerful, positively constructed groups and burdens to less powerful, negatively constructed ones.

## Benefits, Burdens, and Target Populations

The dynamic interaction of power and social constructions leads to a distinctive pattern in the allocation of benefits and burdens to the different types of target groups (Figure 2). The front of the box shows how benefits are allocated, and the back shows the allocation of burdens. Benefits are expected to become oversubscribed to advantaged populations (i.e., these groups will receive more beneficial policy than is warranted either in terms of policy effectiveness or representativeness), whereas dependents and deviants will receive too little beneficial policy. Burdens will become oversubscribed especially to deviants and undersubscribed to the advantaged groups. For public officials to realize their ambitions of reelection and the development of effective, public-oriented policy, they have to take into account not only the power and social constructions of target populations but also the logical connection of the potential target groups to the goals. Most of the time, public officials try to bring these three factors into congruence. It is important to notice that congruence is possible only in two segments of the policy box shown in Figure 2. One is to provide beneficial policy to powerful, positively viewed groups who are logically connected to an important public purpose. The second area of congruence is found at the back of the box: to provide punishment policies to negatively constructed, powerless groups, who are linked logically to a broader public purpose. All other areas produce noncongruence of some type.

Powerful segments of the population who also have relatively consensual positive social constructions (the advantaged groups) have considerable control and will find it easy to get their issues on legislative agendas. They will be the recipient of much beneficial policy. Advantaged groups have the resources and capacity to shape their own constructions and to combat attempts that would portray them negatively. The easiest problems for elected officials to address will be those for which advantaged segments of the population are the logical recipients of beneficial policy. These groups will receive beneficial policy, however, even if the causal linkages to some ostensible common or public purpose lack credibility or are entirely absent. The advantaged groups will often be chosen as first-order (proximate) targets even when others would be more logical or efficient. Direct government subsidies to large corporations, for example, have been granted by governments for the ostensible purpose of increasing the number of jobs in the community, although such funds may have created more jobs if directed toward public-sector agencies with lower management salaries and overhead. Beneficial policy for the advantaged groups will be oversubscribed in the sense that there will be more positive rules and more expenditures in this area than can be justified either on technical grounds of policy effectiveness or on representational grounds of policy responsiveness that is proportional to the group's size and other political resources.

The attractiveness of policy directed toward powerless people with negative images (the deviants) is surprisingly similar except that the deviants are punished and have almost no control over the agenda or the designs. Policies will be high on the legislative agenda, especially during election campaigns. Negatively constructed powerless groups will usually be proximate targets of punishment policy, and the extent of burdens will be greater (oversubscribed) than is needed to achieve effective results. The negative social constructions make it likely that these groups will often receive burdens even when it is illogical from the perspective of policy effectiveness. The highly predictable popularity of tough criminal justice statutes over which deviants have no control, such as the 1991 federal crime bill, are vivid illustrations of the political attractiveness of punishment directed at powerless, negatively viewed groups.

Important public issues do not always permit elected officials to find congruence among social constructions, power, and logical connections to goals; and problems cannot always be solved so straightforwardly. Many officials care about outcomes and fear widespread public reaction against ineffective policy, lack of attention to important problems, and too much favoritism to special interests. They may confront these contradictions through strenuous efforts to keep such issues off the agenda, or they may manipulate the image of target groups in an effort to change their social construction. In some instances, they simply bear the political costs of inflicting burdens on positively viewed groups or granting benefits to those who are negatively viewed. Not uncommonly, public officials engage in private politics or outright deception.

The case of powerful but negatively viewed groups (the contenders) presents numerous problems. Public officials will prefer policy that grants benefits noticed only by members of the target groups and largely hidden from everyone else. They will prefer policies that the public and media believe inflict burdens on powerful, negative groups but that actually have few, if any, negative effects. Contenders have sufficient control to blunt the imposition of burdens but not enough power to gain much in terms of visible benefits. Statutes directed toward these contending groups will be complex and vague. It may be difficult to discern from the statute who the policy favors or hinders because discretion and responsibility will often be passed on to lower-level agencies and governments. Context will become especially important. For example, policy characteristics for contending groups may depend on the extent of media and public attention, as well as variation in the cohesiveness and activity of the target group. During times of low public attention and high levels of group activity, policy will tend to be beneficial, although relatively low in visibility and still undersubscribed in terms of what might be needed to actually solve particular problems. When public attention increases (as it is likely to do when an unpopular group is cohesive and active), then policy may shift more toward the burdensome side.

For the dependent groups, such as children or mothers, officials want to appear to be aligned with their interests; but their lack of political power makes it difficult to direct resources toward them. Symbolic policies permit elected leaders to show great concern but relieve them of the need to allocate resources. Policies in this area tend to be left to lower levels of government or to the private sector. The benefits dependents receive are passed down by other agents, and dependents have little control over the design of the policies. In the United States, women and children have dominated this category, with women moving more toward a position of power (and less positively viewed) as they have become more organized and more active in the economic sector; and people in these groups have been viewed as the responsibility of families, churches, and the private sector. Feminist writers, in fact, view the artificial separation of the public and private spheres as one of the key problems faced by women in advanced industrial democracies (Jagger 1983).

Another type of noncongruence occurs when legislators are attempting to inflict regulations or costs on powerful, popular groups. These situations also will be undersubscribed and highly contentious. For example, it is difficult to generate support for burdensome regulations of positively viewed businesses because the proximate target groups will oppose the policies vigorously and argue that the chain of effects is not likely to produce the desired results anyway; or they may argue that other groups are more logical targets and would have a greater impact and if chosen. The secondary or remote target groups that will presumably benefit from the regulations may not provide as much support as expected, because of the uncertainty that the cause-and-effect logic within the policy is correct (Arnold 1990). In a similar way, it is difficult for elected leaders to provide beneficial policy to the powerless, negatively viewed groups (such as providing rehabilitation programs for criminals), despite the fact that these policies may be more effective than those that involve punishment or may be less costly than the death penalty, given the extensive appeals that ensue. The electoral costs are extensive, as it is a simple matter to accuse a public official of being "soft on crime." Much of the beneficial policy achieved by the powerless, negatively constructed target groups has been through court actions and court mandates to ensure their rights.

### Social Constructions and Policy Tools

The emerging literature on policy design emphasizes that the attributes of statutes, guidelines, implementation structures, and direct service delivery processes are important to an understanding of the policy process. There is considerable interest in why some designs are chosen, rather than others, and what differences these choices make in policy impacts on target populations (Dryzek 1990; Ingram and Schneider 1991; Linder and Peters 1985; Lipsky and Smith 1989; Salamon and Lun 1989; Schneider and Ingram 1990a; Smith and Stone 1988). The theory advanced here contends that some elements of design (especially the policy tools and the policy rationales) will differ depending on the social construction and political power of the target population.

Policy tools refer to the aspects of policy intended to motivate the target populations to comply with policy or to utilize policy opportunities (Schneider and Ingram 1990b). For groups that are constructed as deserving, intelligent, and public-spirited (as we expect the powerful, positively viewed groups to be), the policy tools will emphasize capacity building, inducements, and techniques that enable the target population to learn about the results of its behavior and take appropriate action on a voluntary basis. When delivering beneficial policy to the advantaged groups, certain types of capacity-building tools are expected to be commonly used, especially direct provision of such resources as entitlements or non-income-tested subsidies, and also of free information, training, and technical assistance. The political payoffs for providing beneficial policy to these groups is such that outreach programs will be common: the agencies will seek out all eligible persons and encourage them to utilize the policy opportunities that have been made available (Ingram and Schneider 1991).

When burdens, rather than benefits, are directed at the advantaged groups, the tools will be less predictable and more likely to change; but self-regulation that entrusts the group to learn from its own behavior and voluntarily take actions to achieve policy goals will be preferred, along with positive inducements. When these are not effective in inducing the desired behavior, policies may shift toward "standards and charges," which do not stigmatize the organization for its activities but simply attempt to discourage certain actions (such as pollution) by charging for it. Sanctions and force are not likely to be used in connection with powerful, positively viewed groups.

Policy tools for dependent groups (such as mothers or children) are expected to be somewhat different. Subsidies will be given, but eligibility requirements often involve labeling and stigmatizing recipients. Subsidies to farmers do not require income tests, for example; but college students must prove that they are needy and without resources. Outreach programs will be less common, and many programs will require clients to present themselves to the agency in order to receive benefits. Welfare programs even for persons perceived as deserving, such as college students, the disabled, or the unemployed, usually do not seek out eligible persons but rely on those who are eligible to make their case to the agency itself.

Symbolic and hortatory tools will commonly be used for dependent groups even when the pervasiveness of the problem would suggest that more direct intervention is needed. Groups in the dependent category will not usually be encouraged or given support to devise their own solutions to problems but will have to rely on agencies to help them. For example, battered women still must rely mainly on the police for assistance, rather than having self-help organizations that are eligible as direct recipients for government grants.

Another policy tool, the use of authority (defined as statements that grant permission, prohibit action, or require action) will be more common than with the powerful, positively viewed groups, because dependents are not considered as self-reliant. The so-called gag rule imposed by the Bush administration that prohibited family planning clinic personnel from providing information about abortion even when asked directly was an example of the more paternalistic attributes of policy directed at dependent populations. Information tools are likely to be used, even when direct resources are needed (as in AIDS prevention programs). Public officials simply do not like to spend money on powerless groups and will use other tools whenever possible.

The dominant tools for deviants (the target populations whose constructions place them in the powerless, negatively viewed part of the matrix) are expected to be more coercive and often involve sanctions, force, and even death. In contrast with the kinds of regulations used when advantaged populations are burdened, groups constructed as deviants will be, at worst, incarcerated or executed. At best, they will be left free but denied information, discouraged from organizing, and subjected to the authority of others—including experts—rather than helped to form their own self-regulatory organizations. For example, gangs are more likely to be punished for congregating than encouraged to direct their energy toward constructive activities.

When beneficial policies are directed at deviant groups, such as rehabilitation programs, they ordinarily attempt to change the person through authoritarian means, rather than attack the structural problems that are the basis of the problem itself. Drug diversion programs, for example, will usually require attendance and drug testing, and threaten participants with heavy penalties for failure to comply with the rules.

### Social Constructions and Policy Rationales

Rationales are important elements of policy design because they serve to legitimate policy goals, the choice of target populations, and policy tools. As Habermas and Offe have noted, modern governments have a legitimation crisis and must explain why democracies concentrate wealth and power in the hands of the few rather than the many (Habermas 1975; Offe 1985). Governments attempt to resolve this crisis through legitimation rationales that explain how policies serve common rather than special interests (in spite of appearances). Rationales justify the agenda, policy goals, selection of target populations, and the tools chosen. The kinds of rationales differ depending upon the social construction of the target population and can be used either to perpetuate or to change social constructions.

For powerful, positively viewed groups, the rationales will commonly feature the group's instrumental links to the achievement of important public purposes, currently conceptualized in terms of national defense and economic competitiveness. Justice-oriented rationales (e.g., equality, equity, need, and rights) will be less common for this group. Efficiency as a means for achieving the instrumental goals of policy will be emphasized as the reason for the selection of particular target groups and particular tools. For example, federal science and technology policy, which distributes more than $75 billion annually to large corporations and universities, is justified on the grounds of national defense and/or economic competitiveness. The groups chosen are said to be an efficient mechanism for ensuring the United States maintains its technological edge vis-à-vis other countries.

Similar rationales are used even when burdens are being distributed. The close association of the welfare of these groups with the public interest is not challenged. Instead, groups may be told that they are not being made relatively worse off, compared with their competitors and that all will gain in the long run. Policies to control common-pool resource problems, such as water and air, usually claim that it will protect the resource for everyone and that the regulations will prevent a single firm within their group from gaining advantages and depleting the resource. In those cases where it is impossible to construe a burden as a benefit, then the rationale may claim that it is technically unavoidable if the common-interest goals (e.g., national defense) are to be served. The burden impinges on everyone, and it is not practical to make an exception for the advantaged groups. The advantaged are not being singled out, and they are sacrificing for the public good.

For contending groups (those that are powerful but have negative constructions), the rationale is sharply different, depending on whether they are receiving benefits or burdens. When they are receiving costs, the public rationale will overstate the magnitude of the burden and will construe it as a correction for their greed or excessive power. On the other hand, private communications may suggest that the burden is not excessive or will have little impact. In situations where the burden is real, the group may be led to believe that they did not have enough power or made errors in their strategies. They may be told that the policy was inevitable once public attention was directed to their privileged, powerful position. When contending groups receive benefits, the rationales will understate the magnitude of the gain, which is made easier because the gains often are cloaked as procedures that enable the group to have privileged access to lower-level agencies or governments where the elected officials will not be held accountable for the groups' gains. When the benefits are obvious and can credibly be linked to instrumental goals, such as national defense, arguments will be made that it would not be possible to achieve the goal without also benefiting the group.

Rationales for providing beneficial policy to powerless groups seem to emphasize justice-oriented legitimations, rather than instrumental ones. During the past two decades, the interests of dependent populations have seldom been associated with important national purposes. The association of justice-oriented rationales to dependent populations seems to hold even when a case can be made linking the policy to national goals such as economic development or national defense. Education is a good example. In spite of strenuous efforts by educators to claim that education is the fundamental basis for economic competitiveness (and in spite of the logic of this position), political leaders in the 1980s tended to ignore this connection. Public education has been justified in terms of equal opportunities—a rationale that currently does not carry the same status as instrumental ones. The values of American society simply seem to favor instrumental goals over justice-oriented goals. It may be the case that instrumental goals are given primacy mainly because this permits policy to continue distributing benefits to those who are more powerful. Similarly, elected officials may not want to use instrumental justifications for policies that benefit less powerful people, even when it would be perfectly logical to do so, as this would then require larger expenditures on such groups. Benefits conferred on negatively viewed powerless groups, such as criminals, are frequently argued as unavoidable in order to protect important constitutional principles that confer rights on everyone. Sometimes claims will be made, however, that beneficial policies (e.g., rehabilitation for criminals) are efficient mechanisms for achieving public safety. This argument is difficult to sustain, however, because the public believes that these people deserve to be punished and that rehabilitation policies will not work to reduce crime. Part of the social construction of these groups is that they respond mainly to punishment.

Burdens for powerless groups who are positively constructed, such as children, may be justified as an efficient mechanism to protect the individual from harm or to achieve public purposes. For powerful groups, choices are limited only when there is no other way to achieve certain goals. Persons in the powerful groups are constructed as intelligent and able to make good choices. Powerless groups are not usually constructed this way but are viewed as needing direction. "For her own good" is a common reason given for incarcerating girls who have run away from home or who are living with a boyfriend. Child labor laws that removed choices from children and their families were done to protect the children.

### Messages, Orientations, and Participation

The agenda, tools, and rationales of policy impart messages to target populations that inform them of their status as citizens and how they and people like themselves are likely to be treated by government. Such information becomes internalized into a conception of the meaning of citizenship that influences their orientations toward government and their participation. Policy teaches lessons about the type of groups people belong to, what they deserve from government, and what is expected of them. The messages indicate whether the problems of the target population are legitimate ones for government attention, what kind of game politics is (public-spirited or the pursuit of private interests), and who usually wins.

Citizens encounter and internalize the messages not only through observation of politics and media coverage but also through their direct, personal ex-

TABLE 1

**Policy Design Impacts on Different Target Populations**

| TYPES OF IMPACTS | TYPES OF TARGET POPULATIONS | | | |
|---|---|---|---|---|
| | ADVANTAGED | CONTENDERS | DEPENDENTS | DEVIANTS |
| Messages | | | | |
| Personal | good, intelligent | controversial | helpless, needy | bad |
| "Your" problems are | important public problems | in conflict with others' interests | the responsibility of the private sector | your own personal responsibility |
| Government should treat you | with respect | with fear or caution | with pity | with disrespect or hate |
| Orientations | | | | |
| Toward government | supportive | suspicious, vigilant | disinterested passive | angry, oppressed |
| Toward own interests | coincide with the public interest | conflictive with others | private responsibility | personal responsibility |
| Toward other's claims on government | not legitimate | competitive rivals | more important | simply privileges |
| Toward political game | open, fair, winnable | involving raw use of power and crooked | hierarchical and elitist | abusive of power and fixed |
| Participation | | | | |
| Mobilization potential | | | | |
| for conventional forms (voting, interest groups) | high | moderate | low | low |
| for disruptive forms (strikes, riots) | low | moderate | low | moderate |
| for private provisions of services | high | moderate | low | low |
| Citizen–agency interaction | agency outreach | targets subvert implementation | client-initiated contacts | avoidance |

periences with public policy. These experiences tell them whether they are viewed as "clients" by government and bureaucracies or whether they are treated as objects. Experience with policy tells people whether they are atomized individuals who must deal directly with government and bureaucracy to press their own claims or participants in a cooperative process joining with others to solve problems collectively for the common good. Citizen orientations toward government impinge on their participation patterns.

The personal messages for the positively viewed, powerful segments of society are that they are good, intelligent people (Table 1). When they receive benefits from government, it is not a special favor or because of their need but because they are contributing to public welfare. For these groups, reliance on government is not a signal that they cannot solve their own problems. Government appears responsive to them, and a clear message is sent through the tools and rationales that their interest coincide with the public interest. Policies often involve outreach and seldom require needs tests; thus the advantaged do not see themselves as claimants or as dependent on government. Instead, they are a crucial part of the effort to achieve national goals, such as national defense or economic vitality. When they are regulated, they examine rationales closely to see whether burdens are equitably allocated and whether their sacrifice is truly necessary for a public purpose. When other groups are singled out for benefits, especially those who are less powerful or negatively constructed, they tend to believe that the government is on the wrong track. Advantaged groups are quick to sense favoritism whenever groups other than themselves receive benefits.

Advantaged groups are positively oriented toward policy and politics, so long as government continues to be favorable toward them (which becomes difficult in a troubled economy). Experiences with policy teach them that government is important, politics is usually fair, government can be held responsible for producing beneficial policy, there are payoffs from mobilizing and supporting government officials. The game can be won within the rules. The powerful, popular groups are active participants in traditional ways, such as voting, interest group activity, campaign contributions and so forth. When policies are ineffective, especially when there are sustained periods of economic problems, they blame government rather than themselves and they mobilize for change. When government no longer benefits them, these groups are likely to organize and devise private alternatives to public services, such as private schools, security systems, mental health services, and so on. And, they object even more strenuously to government regulation or to government providing benefits to others. As they increasingly provide services for themselves, they withdraw support for government provision of such services to others, thereby contributing to an ever-widening gulf in the quality of life experienced by the haves and have-nots in modern American society.

Contenders receive different messages. Policy tells them that they are powerful, but they will be treated with suspicion rather than respect. Their power is meaningful only when accompanied by a strategy that will hide the true effects from public view. Politics is highly contentious; no one will take care of them except themselves. Thus, they must use power to pursue their own interests. Contenders realize that conflict is common. They must be constantly vigilant and calculating to insure that government serves their ends. They believe that government is not really interested in solving problems but in wielding power. The difference between the public and private messages that government sends to these groups teaches them that government is not to be trusted. Private power is more important than public interests and rationales are simply subterfuge rather than valid arguments justifying the distribution of benefits and costs. Politics is a corrupt game; winners have successfully used power and may have not stayed within the rules of the game. Participation patterns tend toward the use of informal means, such as the use of influential connections and campaign contributions. Participation may disregard the rules or laws; manipulation and subterfuge are common.

The messages to dependents are that they are powerless, helpless, and needy. Their problems are their own, but they are unable to solve them by themselves. Policy teaches them that it is not in the public's interest to solve their problems, and they get attention only through the generosity of others. To be forced to depend upon a safety net means one is not much of a player. The tools and rationales imply that government is responsive to them only when they subject themselves to government and relinquish power over their own choices. Income testing and the typical requirement that they must apply to the agency for benefits (rather than being sought out through outreach programs) require them to admit their dependency status. Even when beneficial policy is provided, it is accompanied by labeling and stigma. Policy sometimes attempts to overcome negative stereotyping by replacing one label with another, such as using *disabled* instead of *handicapped*, which, itself, was used as a replacement for *crippled*. Unfortunately, stigma often catches up with the new label. Information programs that rely on propaganda and stereotypes for effectiveness primarily reinforce the prevailing social constructions. Efforts to reduce the spread of HIV by appealing to young black males through sport figures such as Magic Johnson may reinforce the image of young blacks as sexually promiscuous.

The messages result in orientations toward government characterized by disinterest and passivity. In contrast with the advantaged groups, the powerless (even when positively constructed) do not see their interests as coinciding with an important public goal and, instead, tend to buy into the idea that their problems are individual and should be dealt with through the private sector. They may view the claims of others, especially the powerful advantaged groups, as being more legitimate than their own. The game of politics is a bureaucratic game where they wait in line and eventually get what others want them to have. Participation is low and conventional, but their primary form of interaction with government is as applicants or claimants who are applying for services to a bureaucracy.

Persons who are both powerless and negatively constructed will have mainly negative experiences with government, but differences in the tools and rules will lead to different messages from those received by other groups. The dominant messages are that they are bad people whose behavior constitutes a problem for others. They can expect to be punished unless they change their behavior or avoid contact with the government. Accordingly, these people often fail to claim government benefits for which they are eligible. On the other hand, government often is unable to catch them for their misdeeds and commonly fails to punish even when individuals are apprehended. Thus, government appears to be arbitrary and unpredictable. The rule of law and justice have no meaning. Orientations will be those of angry and oppressed people who have no faith in government's fairness or effectiveness. They see themselves as alone and as individual players who have no chance of winning in a game that they view as essentially corrupt. Conventional forms of participation such as voting, running for office, and interest group activity will be viewed as irrelevant (even if they are eligible) because government belongs to someone else. Participation, when it occurs, is likely to be more disruptive and individualized, such as riots and protests. As with the contenders, the deviants are more inclined to break the rules of participation.

## The Dynamics of Social Constructions

Social constructions are manipulated and used by public officials, the media, and the groups themselves. New target groups are created, and images are developed for them; old groups are reconfigured or new images created. One of the most interesting questions is whether inherent contradictions within the policy process itself will lead to cyclical patterns of corrections in the over- and undersubscription to different target groups.

One possibility is that beneficial policy becomes increasingly oversubscribed to the advantaged groups, with a corresponding decline in resources available for policy that actually will be effective in achieving public purposes. Government can be expected to continue putting forth justifications claiming that providing benefits to advantaged groups serves broader public interests, but the credibility of these explanations will decline for several reasons. First, personal experiences of ordinary citizens will lead many to realize that policy is ineffective in solving problems, or important problems are not even being addressed, or that the designs of policies are illogical and not actually intended to serve the stated goals. Personal observation and experience will also verify that the democratic image of equality is too far at odds with the actual distribution of benefits, influence, power, and the like. It becomes difficult to continue constructing groups that are overly advantaged in a positive light; similarly, it becomes difficult to continue pretending that the most important goals of society exclude benefits to the growing numbers of seriously disadvantaged groups, particularly when ordinary citizens encounter these people, such as the unemployed, in their daily routines.

In addition to personal experiences, another impetus for doubting the prevailing rationales may be forthcoming from the images portrayed by the media, movies, literature, music, and other carriers of social constructions. These respond to many stimuli, including the creative imaginations and critical skills of artists, writers, journalists, academics, and others. Carriers of social constructions may begin to portray the advantaged segments as greedy, rather than deserving. Dramatic events will often serve as catalysts for changes in social constructions. When powerful, positively viewed groups become construed negatively, the dynamics of policy change dramatically. Some of the previously advantaged groups are displaced into a negatively constructed group that will not be able to garner as much beneficial policy. Other groups that were previously negatively constructed or who had not previously exercised power proportionate to their size (because of the social constructions) may move into the positively viewed, powerful segment. If so, understandings of the public interest may shift to those closer to the interests of previously disadvantaged persons.

The political advantages for inflicting punishment upon powerless, negatively viewed groups are so great that this area also will become oversubscribed and extended to ever-larger segments of the population. It is likely that certain kinds of behavior, such as the use of alcohol or other drugs, will be proscribed simply because the groups who are heavy users are negatively constructed and lack sufficient power to oppose the policies. As these prohibited behaviors spread to more powerful and more positively constructed groups, however, they will eventually reach a number of people whose experiences will not permit them to buy into the messages that they are bad and undeserving people. When common behaviors of large numbers of ordinary people become subject to negative stereotyping and punishment is threatened, the expected acquiescence is unlikely. Instead, these groups may refuse to accept the negative social constructions, mobilize, and engage in widespread political participation, including conventional forms, as well as disruptive behavior such as demonstrations or riots. The cycles of disruptive politics in the United States such as occurred in the 1930s and 1960s may be explained by this dynamic process.

In a relatively open, democratic society, these phenomena might produce pendulumlike cycles of policy that distribute benefits and burdens to differing segments of the population, so that the advantages enjoyed by the powerful, positively viewed groups do not escalate in a linear fashion but are occasionally pulled back. Similarly, the oppressiveness of policy to deviant groups may not continually escalate but may reverse direction toward more benign postures.

On the other hand, there may be no inherent dynamic that produces a cyclical pattern. Changes may be unrelated to the prevailing distribution of advantages and, instead, depend upon opportunities, unexpected dramatic events, and the skills of those who manipulate images and constructions. Still a third possibility is that the advantaged continue to gain at the expense of others and that more and more groups are constructed as deviants and subject to punishment. This process is not self-correcting, because social constructions become increasingly important and difficult to refute (Edelman 1988). Thus, they are manipulated and used to build support for the increasingly uneven allocation of benefits and burdens by government.

## IMPLICATIONS AND APPLICATIONS

An understanding of social constructions makes important contributions to many different issues in political science, three of which will be discussed here: Who benefits and who loses from government action? Who participates? and What is the effect of policy on democracy?

### Beneficiaries and Losers

The framework makes an important contribution to the issue of which groups will benefit from policy—why powerful groups do not always win—and offers a compelling explanation for the prominent role played by punishment in the United States political process. A great deal of research by political scientists has verified that policy often reflects the interests of powerful constituent groups. Theories of self-interested behavior by the groups combined with reelection motivations by elected officials offer possible explanations. As a number of authors have pointed out, however, policy often serves public interests (more commonly than is usually acknowledged by political science), which is far more difficult to explain (Arnold 1990; Kelman 1987). Arnold's theory is that public officials develop strategies based on expectations of how the public will react and that they believe that their opponents and the media can easily arouse the inattentive publics by focusing on policy failures or other errors in judgment. Thus, policy directed solely to the benefit of powerful groups could become a major campaign liability. Arnold anticipates the importance of social constructions of target populations when he notes that politically repellent options "also include programs for which citizens have little sympathy for the affected groups" (1990, 80). Kelman simply asserts that public spiritedness is as important a motivation for behavior as self-interest. Thus, elected officials are motivated sometimes by self-interest, producing policies benefiting powerful groups in the constituency but sometimes by public spiritness, producing good public policy that serves general interests (Kelman 1987).

Social constructions make an important contribution to these explanations. Social constructions of target populations help explain the kinds of issues that opponents and media can exploit, namely, any policy that confers benefits on negatively constructed groups (as is illustrated in the Willy Horton ads) or policies that confer burdens on positively constructed groups. The tensions created by noncongruity among social constructions, power, and logical relationships create many situations in which elected leaders will distribute benefits and burdens outside the dictates of power. Furthermore, social constructions are essential to an explanation of the politics of punishment, which wins no votes among the recipients of punishment and appears to accomplish few, if any, positive purposes.

## Who Participates?

One of the enduring issues in political science is why participation is so low and uneven. Many have pointed out that the groups who stand to gain the most from political action, such as the poor and minorities, often fail to mobilize and, in fact, have the lowest rates of participation. Some theorists have examined the importance of structural impediments, such as voting registration rules; others have emphasized that the typical political agenda may be irrelevant to the disadvantaged groups or that the disadvantaged may find it difficult to recognize their own interests as being sufficiently distinct to warrant active participation (Gaventa 1980; Piven and Cloward 1988). Some critical theorists have suggested that the wants and desires of disadvantaged groups are manipulated by the powerful through appeals to symbols, thereby leading to quiescence (Gaventa 1980; Luke 1989). Others have advanced the theory that politics becomes increasingly technical and that government offers complex, technical explanations for policy designs that are beyond the comprehension of everyone except the experts (Fischer 1990; Habermas 1975; Hawkeswork 1988). The result is a depoliticization of society and a withdrawal of citizens from political discourse and activity.

The concept of social construction of target populations helps explain how and why these linkages occur. Policy is an important variable that shapes citizen orientations and perpetuates certain views of citizenship that are in turn linked to differential participation among groups. Groups portrayed as dependents or deviants frequently fail to mobilize or to object to the distribution of benefits and burdens because they have been stigmatized and labeled by the policy process itself. They buy into the ideas that their problems are not public problems, that the goals that would be most important for them are not the most important for the public interest, and that government and policy are not remedies for them. They do not see themselves as legitimate or effective in the public arena, hence their passive styles of participation. In contrast, the advantaged groups are reinforced in pursuing their self-interests and in believing that what is good for them is good for the country. They can marshal their resources and use them to gain benefits for themselves, all the while portraying themselves as public-spirited. Others do not object, and in fact, support such policies, because they accept the goals that benefit the advantaged groups and believe these groups are deserving of what they get. Social constructions enhance their power, whereas it detracts from the power of the disadvantaged groups.

## Policy and Democracy

Social constructions of target populations are crucial variables in understanding the complex relationship between public policy and democratic governance. The theory presented here is an extension of the work of Lowi, Wilson, and others who are interested in how policy affects democracy. It offers explanations for some of the incorrect predictions from Lowi and Wilson's typologies and implies different prescriptions about what is needed for policy to serve democratic roles in society (Barber 1984; Lowi 1964, 1972, 1985; Wilson 1973, 1986).

Lowi popularized the idea that "policy creates politics," turning political science away from its almost exclusive attention to how "politics creates policy." His concern was to identify the attributes of policy that encourage affected people (or groups) to mobilize, to make their preferences clear, and to ensure that policy reflects compromises among competing interests rather than the influence of a small number of elites. Lowi's typology was based on two dimensions: whether the probability of coercion is low (benefits distributed) or high (costs distributed) and whether the policy identifies specific targets or consists of general rules that impinge on the environment of the target groups. These two dimensions produce four types of policy—*distributive, regulatory, redistributive,* and *constituent*—of which only one, regulatory, produces political activities resembling an open, competitive model of pluralist democracy. All of the others, he argued, encourage some type of elitism. Wilson's typology also was developed to explain how and why different kinds of policies produce different kinds of politics. His typology accounted for four types of politics: *majoritarian, pluralist, elitist,* and *client,* depending on whether the benefits and costs are concentrated or dispersed (Wilson 1986).

Social constructions add to both these theories in several ways. Lowi was especially opposed to distributive policy arenas, which are characterized by distribution of beneficial policy directly to constituent groups, because these tend to produce a pattern of mutual noninterference and sub rosa decision making in which only the few participate and only the few are served (Lowi 1979). Social constructions add to this by explaining why some groups are regularly singled out for distributive policy, whereas others are not. It is not simply a matter of power, assessed in traditional ways such as size, wealth, cohesion, and the like. Nor is it simply a matter of concentrated benefits and dispersed costs, as Wilson's typology suggests. Distributive policy is most likely to be directed at target populations that are both powerful and positively constructed. When unpopular groups, including those that are powerful, are targeted for distributive policy, Lowi's predictions of low conflict and mutual noninterference are usually incorrect. Instead, opposition emerges, so that the policy arena resembles the one Lowi characterized as redistributive or regulatory. When groups that lack power but have positive constructions are targeted for distributive policy, opposition also will emerge. In addition, some regulatory policy does not produce opposition (as Lowi's theory suggested) but is met instead with general approval. Lowi's typology clustered social regulation (e.g., crime policy) with business regulation because both inflict coercion on general categories of people. Yet, punishment-oriented crime policies are almost never met with the type of pluralist opposition that characterizes business regulatory policy.

Social constructions also help explain anomalies in predictions from Wilson's theory. For example, welfare policies are characterized by concentrated benefits and dispersed costs—the type of policy that Wilson contended will continually expand, because those who benefit will mobilize, whereas those who pay (the taxpayers) will not. Thus, elected officials who are motivated by reelection will be unable to cut or reduce these kinds of policies. Social constructions help explain why (and when) elected officials will find it easy to cut welfare policies, as has happened in many states during the past decade when the poor were constructed as lazy or shiftless and were often believed to be minorities who were responsible for their own plight.

For Lowi, policies that serve broad public purposes contain a clear rule of law applicable to broad categories of people and contain clear and consistent directives are most likely to produce an environment in which democracy can flourish. Yet as Ginsberg and Sanders point out, such laws dignify and empower only the individuals who know what the law is and can effectively challenge arbitrary and unjust treatment (1990, 564–65). Powerful, positively constructed groups continue under such policies to be reinforced in the belief in their own deservedness and association of their self-interest with the general interest. Groups negatively socially constructed will continue to see government as a source of problems, rather than solutions, and participation as an irrelevant activity. True empowerment and equality would occur only if all target populations had social constructions that were positive and only if all have power relatively equal to their numbers in society.

A theory of the social constructions of target populations is also relevant to an understanding of policy failure in the technical sense of policy that is not effective or efficient. Policy scientists have typically blamed policy failures on illogical linkages in the policy design and have blamed these illogical connections on elected officials who pay too much attention to powerful interest groups and not enough attention to experts (Brewer and deLeon 1983; Quade 1982). A theory of the social construction of target populations makes it clear that policies are not technically illogical simply because of political power considerations. Social constructions are crucial to understanding which policies are most likely to be illogical. Social constructions impinge on all aspects of design, including selection of goals, targets, tools, and implementation strategies. Experts do not escape social constructions, either; and the constructions they hold color which goals they think are important and which targets they believe are the most logically connected to the goals. The tools that experts think will motivate the targets rest on assumptions about behavior that are influenced by social constructions. The rationales that the experts believe will make the policy palatable to affected groups imply particular social constructions of those groups. Thus, social constructions (as well as power) influence the logic of policy, and expertise does not negate the influence of constructions on policy design even in highly nonpolitical contexts.

One of our fundamental contentions is that policies that fail to solve problems or represent interests and that confuse, deceive, or disempower citizens do not serve democracy. Policy designs that serve democracy, then, need to have logical connections to important public problems; represent interests of all impinged-on groups; and enlighten, educate, and empower citizens. Policy should raise the level of discourse. Given the electoral dynamics described here, however, it is not likely that policy will be designed to achieve all three of its democratic roles unless the power of target populations is made more equal and social constructions become less relevant or more positive. In other words, the only groups in the policy typology for which policy is likely to serve democratic roles are the powerful, positively constructed groups. Until all groups are so situated, policy will continue to fail in its democratic mission.

## CONCLUSION

Political scientists should include the social construction of target populations among the political phenomena to which they devote their research. Social constructions are political in the sense that they are related to public discourse and are manipulated through hortatory and symbolic language generally regarded as political. Further, while not discussed in detail here, social constructions are measurable through familiar survey methods, as well as historical and textual analysis.

Social constructions of target populations help provide better answers to Lasswell's (1936) enduring question, Who get what, when, and how? Conventional political science hypotheses about the characteristics that determine groups' influence in setting policy agendas and influencing policy content become significantly more robust when augmented by assessments of social constructions. Further, understanding social construction of target populations helps to explain how elected officials behave and why, in some circumstances, officials will support policy provisions that distribute benefits at odds with their apparent self-interest, as determined by their assessment of interest group and constituency opinion. The concept facilitates a much more sophisticated assessment than has so far taken place concerning the extent to which public officials are motivated to solve substantive problems, as well as build and maintain political support.

The inclusion of social constructions of target populations resolves some of the differences among theories that relate characteristics of policies to patterns of policymaking, including those of Lowi and Wilson. Social constructions of targets help us to understand the dynamics of policy change, even in policy arenas such as the distributive one (which previous theory predicts will be stable).

Concern with social constructions of target populations amplifies the justification for political scientists to study policies and strengthens their credentials as policy analysts. Social construction of targets contributes to an increasingly rich elaboration of the characteristics or elements of policy and their effects, which go beyond earlier preoccupation with costs and benefits to include messages about citizenship and government. The impact of policies upon target populations' perceptions of democracy, inclination toward participation, and willingness to comply with policy directives is clearly an appropriate subject for political science analysis. The boundaries of the political science field, which are fluid and constantly changing, must be redrawn to include this promising political concept.

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# RAWLS'S "POLITICAL" PHILOSOPHY AND AMERICAN DEMOCRACY

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*John Rawls has recently argued that political philosophy can significantly contribute to making democratic societies stable. He seeks moral principles that can ground what he calls an* overlapping consensus *and argues that his well-known principles of justice can serve in this capacity. I criticize both Rawls's general claims about the role of political philosophy and his particular defense of the principles of justice. Both arguments commit Rawls to specific empirical claims about existing liberal societies that are highly questionable. In particular, the Kantian moral views that Rawls believes to be central to liberal culture are controverted by extensive empirical research on the actual beliefs of liberal citizens. Despite the problems with Rawls's arguments, I suggest that a rather different overlapping consensus appears to contribute to stable democracies. This centers on support of the political system rather than more substantive moral principles.*

In his recent articles, John Rawls argues that political philosophy can play an important political role in contributing to stable democratic societies (Rawls 1985, 1987, 1988, 1989; see also Rawls 1958, 1971, 1980). He seeks to develop principles that can be accepted by a large majority of liberal citizens in spite of their diverse religious, moral, and political views, thereby establishing what he calls an "overlapping consensus." Rawls assigns this task to the two principles of justice familiar from *A Theory of Justice* (1971, 60, 302–3). In his recent writings, he gives political philosophy's practical task precedence over a more traditional attempt to identify true moral principles. Rawls writes: "The aim of justice as fairness as a political conception is practical, and not metaphysical or epistemological. That is, it presents itself not as a conception of justice that is true, but one that can serve as a basis of informed and willing political agreement between citizens viewed as free and equal persons" (1985, 230; see also Rawls 1980, 519).

I shall examine what Rawls calls his *political* conception of the role of political philosophy. I am particularly concerned with how well it supports the two principles of justice that Rawls has been defending in different ways since the 1950s.[1] As we shall see, his political defense of the principles commits him to specific factual claims about the political culture of liberal societies that are at odds with the findings of extensive empirical studies conducted over the past fifty years. Though I believe that something like Rawls's account of political philosophy can be defended, in terms of substantive moral content this falls far short of the Kantian moral–political view expressed in the two principles of justice. I begin with an examination of Rawls's view of the role of political philosophy in liberal society. I attempt to distinguish his general sociological theses, based on what he calls "the common sense political sociology of democratic societies" (1987, 4, n. 7), from particular contentions about how justice as fairness plugs into this. I shall not take issue with Rawls's major sociological claims and shall even present some empirical evidence to support them. However, substantial evidence has been amassed by social scientists that bears on his specific claims. Not only does Rawls appear to be unaware of these findings, but they are damaging to his view. Although a particular *overlapping consensus* does appear to contribute to the stability of democratic society,[2] in content, this is far removed from justice as fairness and, in fact, verges on the kind of utilitarian principles that Rawls has opposed throughout his philosophical career.[3]

## RAWLS'S "POLITICAL" ARGUMENT

Rawls bases his account of political philosophy's practical role on a series of sociological claims. These are described most fully in his 1989 article, "The Domain of the Political and Overlapping Consensus," on which I draw in this brief summary. Rawls presents four general facts, then adds a fifth. These are as follows.[4]

1. The public culture of modern democratic societies contains numerous, diverse religious, moral, and philosophical doctrines as one of its permanent features. Rawls refers to this as "the fact of pluralism" (1989, 234–5).
2. A continuing, general affirmation of one religious, philosophical, or moral view could be brought about only through the oppressive use of state power.
3. Political stability requires some measure of societal agreement. In Rawls's words, "An enduring and secure democratic regime, one not divided into contending doctrinal confessions and hostile social classes, must be willingly and freely supported by at least a substantial majority of its politically active citizens."[5] Without consensus, a regime "will not be enduring and secure" (1989, 235).
4. The political culture of a reasonably stable democratic culture normally contains a number of intuitive ideas from which it should be possible to

work up "a political conception of justice suitable for a constitutional regime."

5. For various reasons, it is unlikely that rational arguments alone could bring about agreement throughout society on important moral, religious, and philosophical questions. Rawls refers to this as "the burdens of reason" (1989, 235–38). It is because of this that such agreement could be secured only through the oppressive use of state power (see fact 2).

This summary omits many details of Rawls's account but should be adequate for our purposes. Rawls's view of consensus centers on his notion of an *overlapping consensus*. Roughly and briefly, Rawls argues that the practical task of political philosophy is to provide a basis for consensus on essential questions. An overlapping consensus is an agreement on central political issues by individuals who subscribe to conflicting comprehensive moral and religious philosophies. In contrast to general and comprehensive moral views, a *political* view is both narrow and superficial. While a general conception applies to a wide range of moral subjects, a political conception applies narrowly to political, social, and economic institutions, to what Rawls calls "the basic structure of society" (1987, 3). As Rawls uses the term, a comprehensive moral view, like many religious and philosophical views, contains explicit answers to difficult and controversial questions, such as, What is of value in human life? and What is the nature of personal goodness? (1989, 240). A fully comprehensive view "covers all recognized values and virtues within one rather precisely articulated scheme of thought" (1988, 253). A political view, in contrast, stays on the philosophical surface. Rawls believes that proponents of different comprehensive religious and moral views can accept a common set of political precepts, though they would perhaps subscribe to them in different ways. Rather than appealing to controversial religious or moral premises, a political view is grounded on the common stock of intuitive ideas found in a liberal society, though, once again, proponents of different comprehensive doctrines hold these ideas for somewhat different reasons. In order to secure an overlapping consensus, Rawls believes that it is necessary to "apply the principle of toleration to philosophy itself" (1987, 13). By practicing "the method of avoidance," he hopes to secure agreement on basic political principles while avoiding intractable moral and philosophical disputes: "The question is: what is the least that must be asserted; and if it must be asserted, what is its least controversial form?" (1985, 230–31; 1987, 18).[6]

Rawls defends different aspects of this overall view. For instance, he is concerned to rebut charges of moral skepticism (1987, 12–15); to distinguish his political conception of justice from different comprehensive doctrines; and to distinguish an overlapping consensus from a less satisfactory modus vivendi, an agreement between people with different comprehensive views merely to live and let live (esp. pp. 9–12, 18–23). Other aspects of Rawls's view, however, are not clearly elaborated. He places great weight on the claim that "an enduring and secure democratic regime" requires some measure of consensus among a large majority of its citizens (1989, 235). But exactly what constitutes a stable and secure regime? What role does intellectual consensus play in promoting stability, and how does this contribution compare to those of other factors, such as rough equality in distribution of wealth? One can also ask about the nature of the necessary consensus and whether the political culture of modern democratic societies actually contains the fundamental intuitive ideas from which the requisite consensus must be derived? Rawls's discussion of these matters is often abstract and general. Though he devotes considerable effort to distinguishing his position from others with which it can be confused, his account of what his view is not is frequently unaccompanied by an equally clear account of exactly what it is. I must venture answers to some of these questions here and shall also attempt to identify specific factual claims to which Rawls's view commits him.

If political philosophy is intended to secure stable regimes, a great deal depends on what Rawls means by "stable regimes." As this conception varies, the specific conception of political philosophy one upholds will vary with it. It is clear that Rawls sees the desired end as "a stable constitutional regime," "an enduring and secure democratic regime" (1989, 235 and abst.). But beyond these and similar descriptions, exactly what he has in mind must be surmised.

What Rawls means by a stable society is to some extent clear in what it excludes. Obviously, such a regime must not be beset by revolutions, frequent constitutional changes, or civil strife. At one point, Rawls says that it is "not divided into contending doctrinal confessions and hostile social classes" (1989, 235). The desired society is obviously free of the ruinous level of conflict experienced by certain countries in the sixteenth- and seventeenth-century religious wars (to cite an example Rawls frequently uses, e.g., 1985, 225; 1987, 4). Perhaps a useful conception of stability is one employed by modern social scientists. In *Political Man* (originally published in 1960), Seymour Martin Lipset describes as stable democracies countries that maintain "the uninterrupted continuation of political democracy since World War I *and* the absence over the past twenty-five years of a major political movement opposed to the democratic 'rules of the game' " (1981, 30; emphasis original).[7]

It is clear, however, that Rawls has in mind more than mere stability. In addition to this (which we can refer to as the "sociological" component of the political end) is a normative component. Political stability cannot, for example, rest on the oppressive use of force seen in various dictatorships. Rawls's desired regime must be freely accepted by a majority of its citizens (1989, 235). He also strongly supports central liberal values, especially democracy and the protection of rights and liberties (p. 241). In keeping with these values, Rawls wants a regime in which state power is exercised only in ways that all citizens can be expected to regard as reasonable (p. 244).

His attention to these and other similar values raises a possible problem. As we shall see, the method of justification that Rawls supports in his recent work proceeds from the actual views of political actors. Since an overlapping consensus must be acceptable to the broad majority of citizens, the normative side of his political ideal must be rather thin. Beyond a certain point, emphasis on such values as rights and liberties or personal autonomy leaves the domain of the political to verge upon a specific comprehensive moral view that would not be broadly accepted. If members of society cannot agree on a single comprehensive moral view, we cannot expect them to accept a political ideal with extensive normative content. To some extent, Rawls can assume that the citizens of liberal societies support liberal values in spite of their moral, religious, and philosophical differences. Thus, the optimal political end would appear to combine an enduring, stable regime with democratic values, to the extent that these can be freely supported by the bulk of the population.[8]

Concerns of stability underlie Rawls's distinction between an overlapping consensus and a modus vivendi (discussed most fully in 1987, 18–23). A modus vivendi is conceived on the model of a truce. Its terms reflect the balance of power between contending factions at the time it is made and are subject to change as conditions evolve. An overlapping consensus is more secure, because it comes to be viewed as more than a mere means to stability. Very briefly, Rawls appeals to the psychological fact that a suitably working political agreement will develop widespread allegiance. As people cooperate with one another on terms that they regard as fair, they will come to view the rules of cooperation as good in themselves, thus lending them an additional measure of support (1987, 21–22; 1989, 245–48). Rawls makes strong claims for the sociopolitical effects of an overlapping consensus. In various contexts, he seems to regard it as necessary and sufficient for political stability (e.g., 1989, 246), though this perhaps overstates his fully considered position.

In order for an overlapping consensus to develop in a given society, the views it contains must be supported by a majority of inhabitants. Accordingly, it must be based on what Rawls calls *intuitive ideas,* ideas that are implicit in the political culture of democratic societies. In different contexts, Rawls describes these as "embedded in the political institutions of a constitutional democratic regime and the public traditions of their interpretation," "take[n] to be implicit in the public culture of a democratic society," providing the wherewithal "to work up a political conception of justice suitable for a constitutional regime," "recognize[d] as true," and "viewed as latent in the public political culture of a democratic society" (1985, 225, 231; 1987, 4 [n. 7] and 1989, 235; 1987, 6; 1988, 252).

These and other formulations of Rawls are so vague and abstract that it is difficult to know exactly what he means by them. But he is undoubtedly committed to the claim that most liberal citizens would accept a set of beliefs closely related to those he expounds in *A Theory of Justice* and subsequent writings. Rawls undoubtedly believes that intuitive ideas correspond, in some sense, to what people believe and would accept if put to them in a certain way. This is confirmed by his view of how political principles are justified: "Justification is addressed to others who disagree with us, and therefore it must always proceed from some consensus, that is, from premises that we and others publicly recognize as true" (1985, 229; see also 1987, 6). Political principles are not justified by being shown to be true in a "metaphysical" sense, for example, in being deduced from self-evident first principles. Rather, they are justified *to* other people by being shown to stem from premises they accept. In order to fulfill their political role, the requisite principles must be accepted by "at least a substantial majority" of politically active citizens, each of whom endorses them from his own point of view (1989, 239). In order for acceptance to be free and willing (1987, 4, n. 7), there must be a strong correspondence between the content of the principles and subjects' political ideas. Rawls's appeal to intuitive ideas, I take it, distinguishes them from the political views that people consciously hold at a given time. Obviously, if there were a strong correspondence between what people consciously believed and the contents of the two principles of justice, they would easily accept them.[9] An intuitive idea, instead, appears to be one that people are not necessarily aware of holding but to which they are committed because of their other beliefs. In "Kantian Constructivism in Moral Theory," Rawls speaks of "underlying notions and implicitly held principles": "[The] aim of political philosophy, when it presents itself in the public culture of a democratic society, is to articulate and make explicit those shared notions and principles thought to be already latent in common sense" (1980, 518). Confronted with proper political principles, then, people will recognize them as expressing ideas they implicitly hold and accept them for that reason.

This construal is supported by the "reflective equilibrium" method discussed in *A Theory of Justice* (1971, 19–21, 46–53, 577–86; see also 1987, 5, n. 8). Rawls believes that moral theory is "Socratic" (1971, 49). In *A Theory of Justice,* he argues that moral principles are justified by being shown to fit into integrated structures of moral beliefs and principles. Through reflective equilibrium, subjects attempt to attain consistency between their moral principles and opinions about particular cases to which the principles apply. To achieve consistency, they must revise their principles to accomodate the cases of which they are most certain and their views of particular cases to accomodate firmly held principles. One way that consistency is attained is by showing that a number of apparently diverse moral beliefs can be derived from a set of underlying basic principles. In "The Basic Liberties and Their Priority," Rawls argues that "the two principles of justice provide a better understanding of the claims of freedom and equality in a democratic society" than other familiar principles (1982, 6). He attempts to show that the history of constitutional doctrine in the United States in regard to free speech can be organized around principles akin to his principles of justice. Rawls notes the possibility that more than one set of principles "can be worked up from the fund of shared political ideas" in a given culture (1987, 7). Elsewhere, he indicates his desire to locate the single set of principles *most* congenial to his subjects (1980, 517, 534). But however we come down on this particular issue, it is clear that a suitable set of moral principles must be derivable from fundamental intuitive ideas of the subjects in question. It follows, then, that in regard to the actual citizens of liberal societies, Rawls is committed to an important factual claim. Although most citizens may not be aware that they subscribe to the two principles of justice and could well be dubious about them if confronted with them directly, the principles nevertheless represent the implications of moral principles to which they do subscribe.

As I have noted, the content of the two principles of justice is Kantian, especially as epitomized in Kant's injunction always to treat people as ends, never as means (see Rawls 1971, 179–83). As Rawls writes in the opening of *A Theory of Justice*:

> Each person possesses an inviolability founded on justice that even the welfare of society as a whole cannot override. For this reason justice denies that the loss of freedom for some is made right by a greater good shared by others. It does not allow that the sacrifices imposed on a few are outweighed by the larger sum of advantages enjoyed by many. Therefore in a just society the liberties of equal citizenship are taken as settled; the rights secured by justice are not subject to political bargaining or to the calculus of social interests. (pp. 3–4)

In more concrete terms, Rawls's conception of personal inviolability finds expression in the "priority of right." In *Theory of Justice* the first principle of justice (the *equal liberty* principle) is assigned "lexical priority" over the second (the difference principle), which means that claims of liberty must be satisfied completely before other values are invoked: "Liberty can be restricted only for the sake of liberty itself," rather than for other values (p. 244; see also pp. 541–48). The priority of right means that basic liberties "have an absolute weight" in regard to other values (1982, 8). Rawls's understanding of this view has evolved between the publication of *A Theory of Justice* and "The Basic Liberties and Their Priority," in which it is discussed at length. The details of his different positions need not be reviewed here. It should suffice to note his consistent adherence to the inviolability of liberty (or rights, terms I shall generally use interchangeably). Rawls believes that utilitarians can be faulted for allowing trade-offs between liberty and other values, and defines justice as fairness in opposition to utilitarianism (see 1971, secs. 5–6). Rawls believes it preferable to utilitarianism especially on account of its refusal to countenance violations of liberty even when these advance other significant values. Specific liberties that fall under the priority of liberty include political liberties, including the rights to vote, to assemble, and to be eligible for public office, and other, related freedoms, such as freedom of speech and the press and freedom from arbitrary arrest (1971, 61; see also 1982, 23–24). I shall refer to rights and liberties that cannot be traded off for other values as *strong* rights and liberties and to Rawls's view as a *strong* conception of rights and liberties.

It follows from my discussion of intuitive ideas that if the two principles of justice can be derived from fundamental intuitive ideas of liberal political culture, then the inviolability of liberty must be so derivable, as well. Rawls is committed to specific factual claims about rights, especially that his view of strong rights is more "congenial to" liberal political culture than a utilitarian conception that allows trade-offs. Because the views in question are intuitive ideas, Rawls does not have to show that people consciously adhere to a strong conception as things presently stand. But he is committed to the view that strong rights are better able than other views to order, or make sense of, the major tendencies in liberal political culture.

In the light of this unavoidable empirical aspect of Rawls's political defense of the two principles, one would expect him to examine liberal political culture in detail to test his view. With one exception, however, his few recent attempts to do this must be described as perfunctory. For example, in "Justice as Fairness: Political, Not Metaphysical," Rawls speaks of "the important conception of democratic individuality expressed in the works of Emerson, Thoreau, and Whitman" (1985, 246, n. 29). He also notes our "settled convictions" in regard to religious toleration and opposition to slavery, which can be regarded as "provisional fixed points" around which a suitable conception of justice can be constructed (p. 228). But these brief remarks do little to address the variety of conflicting elements present in liberal culture.

The most sustained examination of liberal political culture I have found in Rawls's works is in "The Basic Liberties and Their Priority." He carefully surveys the tradition of constitutional interpretation in regard to allowable restrictions on freedom of speech. He views American political culture as extremely tolerant: "Within our tradition there has been a consensus that the discussion of general, political, religious, and philosophical doctrines can never be censored" (1982, 58). His examination of Supreme Court decisions confirms this impression. He demonstrates the centrality of a strong conception of the right to free speech, which can be restricted only under emergency conditions, when this is necessary to preserve the system of institutions that makes the basic liberties possible: restrictions are justified only if there exists a constitutional crisis "requiring the more or less temporary suspension of democratic political institutions, solely for the sake of preserving these institutions and other basic liberties" (p. 70).

Rawls's examination of the cases is persuasive; and he demonstrates the overall congruence of constitutional doctrine and the priority of liberty in certain respects, though with a few significant exceptions (1982, 74–79). It seems to me that one reason he does not pursue his examination of liberal political culture further is his general faith in its respect for rights, an impression he believes is confirmed by his analysis of constitutional cases.

But once again, Rawls's area of inquiry is selective. Even within the purview of constitutional doctrine, certain of the Court's decisions suggest underlying principles opposed to his (1982, 74–79). It is also doubtful that Rawls would argue that liberal political culture is necessarily epitomized in Supreme Court decisions. It is likely that the Rehnquist Court has a view of rights decidedly less strong than his own, which will show up increasingly in decisions that will disappoint him in coming years. Rawls certainly does not argue that an overlapping consensus should be based on Supreme Court cases alone, while his brief forays into liberal culture overlook an enormous body of evidence against the anticensorship consensus he believes to exist in American culture.[10]

## RIGHTS IN LIBERAL POLITICAL CULTURE

As I have suggested, Rawls's claims about the shared tradition of liberal political culture are immediately suspect. The American political tradition (any political tradition) is diverse, containing conflicting elements. People with different values will identify its most salient elements according to their values and beliefs. Facts along these lines are basic to "the burdens of reason" (1989, 235–38). The claim that the large majority of Americans will view similar elements as central is not easily reconciled with the burdens of reason. Thus, it is especially important for Rawls to examine the liberal political tradition in detail in order to defend the values he believes to lie at its core.

Though Rawls makes little effort to sort out the basic values of the liberal tradition, numerous social scientists have studied these matters extensively. The results of their research are troubling for Rawls, because the pervasive intolerance of liberal citizens is one of the best attested facts of modern social science. A series of empirical studies conducted over the past half-century has established that Americans are surprisingly willing to abridge the rights of different groups. Among the rights they are willing to cast aside are many that fall under Rawls's basic liberties.[11]

Empirical studies of Americans' attitudes about respecting the rights of different groups constitute a rich and diverse literature. I cannot do justice to, or summarize, the breadth of its findings in this brief space. Nor can I discuss theoretical issues concerning the nature of tolerance, how levels of tolerance should be assessed, or the implications of different construals for democratic theory.[12] To a large extent, however, these issues can be bypassed here. On the main point, the findings of researchers have been consistent. Perhaps the best-known study was the survey of American attitudes toward communism and civil liberties conducted by Samuel Stouffer in 1954. Stouffer reports being surprised by some of his findings (1955, 14). However, though these were undoubtedly affected by the troubled period he examined, it should be emphasized that his overall results have been strongly confirmed by subsequent studies. To take one example, Stouffer examined attitudes toward rights to free speech of socialists, communists, and atheists. Survey respondents were asked, "If a person wanted to make a speech in your community favoring government ownership of all the railroads and big industries, should he be allowed to speak or not?" Of the sample, 31% responded *no,* 11%, *no answer,* and 58%, *yes.* Accordingly, 42% of the sample had at least some doubts about socialists' rights to speak (p. 29). In regard to atheism, respondents were asked, "If a person wanted to make a speech in your community against churches and religion, should he be allowed to speak, or not?" Sixty percent responded *no,* 3%, *no opinion,* and 37%, *yes* (p. 33). In regard to communism, the following questions was asked: "Suppose an admitted Communist wants to make a speech in your community. Should he be allowed to speak or not?" To this, 68% responded *no,* 5%, *no opinion,* and 27%, *yes* (p. 41). Clearly, Stouffer uncovered widespread willingness to interfere with the rights of various groups of people.

Stouffer's results can be compared with those of more recent studies. In surveys described by David Barnum and John L. Sullivan conducted in Britain (in 1986) and the United States (in 1978 and 1987), nationwide samples were asked whether they would allow members of groups they especially disliked to (1) make a public speech, (2) hold a public rally, (3) run for public office, (4) form an organization that is not banned or outlawed, or (5) teach in a public school and (6) whether the government should be allowed to tap the telephones of group members (Barnum and Sullivan 1990, 722).[13] The results they report are striking. For instance, 27%, 16%, and 27% would have allowed members of the least-liked group to run for public office in Britain in 1986 and the United States in 1978 and 1987, respectively; 34%, 34%, and 33% would have permitted members of the least-liked group to hold a public rally; 31%, 29%, and 32% opposed banning (outlawing) the least-liked group. Slightly better, 51%, 50%, and 50% would permit members of the least-liked group to make a public speech.

Granting basic rights to all citizens, even those with whom one disagrees, is a fundamental principle of democratic politics and fundamental to Rawls's basic liberties. As Sullivan and Barnum note, four of the activities they address (making speeches, holding rallies, running for office, and organizing groups) are commonly viewed as key attributes of a democratic political system (1990, 723). These also fall under Rawls's basic liberties (1982, 49–50). The unavoidable conclusion of decades of empirical studies, then, is that significant majorities of Americans, like citizens of other liberal societies, do not uphold basic liberties in the strong sense Rawls requires.

These empirical findings are complicated by the fact that respondents voice strong support for democratic rights presented in the form of abstract principles. This conflicts with their responses to specific circumstances and has also been repeatedly confirmed. In a well-known study of voter attitudes in Tallahassee, Florida, and Ann Arbor, Michigan, James Prothro and Charles Grigg (1960) asked their respondents whether they agreed or disagreed with broad principles expressing democracy, majority rule, and minority rights, and then specific applications. The degree of agreement on the broad principles was 94.7%–98%. According to Prothro and Grigg, this "appears to represent consensus in a truly meaningful sense" and supports the idea that "consensus on fundamental principles is essential to democracy" (pp. 284–86, 276). However, "when these broad principles are translated into more specific propositions, consensus breaks down completely" (p. 286). While more than 80% of their respondents opposed barring a legally elected black from office, only 21% supported allowing nontaxpayers to vote. Roughly half the sample supported limiting the right to vote to "well-informed" citizens, barring a communist from elective office, and allowing bloc voting for professional associations, such as the American Medical Association. While 63% would allow an antireligious speech, and 79.4%, a socialist speech, only 44% would allow a communist to speak. Although 75.5% would refuse to bar a black from candidacy for public office, only 41.7% would refuse to bar a communist (p. 285).

In their analysis of two national studies conducted in the late 1970s, Herbert McClosky and Alida Brill present similar findings. On a general level, support for freedom of expression was high. Ninety percent of respondents expressed agreement with the statement "I believe in free speech for all no matter what their views might be." Similar percentages supported other, similar principles (McClosky and Brill 1983, 50). But on the particular level, support dropped off sharply. For instance, only 18% would permit the American Nazi party to use the town hall for a public meeting. Only 23% would allow a group use of a public building to denounce the government (p. 53).

The apparently conflicting results of these studies provides Rawls with a way to explain them. His view does not require that people consciously subscribe to his principles of justice, so it is not surprising that they do not present a strong view of rights in their responses to survey questions. As we have seen, Rawls views moral theory as "Socratic." Confronted with inconsistencies in their moral beliefs, individuals should revise their opinions (1971, 49). Thus, if subjects are shown that their belief that atheists or communists should not be allowed to speak conflicts with their strong adherence to an abstract principle of free speech, they should alter the former opinion. This suggestion is supported by Prothro and Grigg's findings concerning the correlation between levels of toleration and of education. They believe that education leads to toleration by providing individuals "greater acquaintance with the logical implications of . . . broad democratic principles" (1960, 291). According to this line of argument, if Americans were better educated, they would revise their opinions about the permissibility of violating minority groups' rights.[14]

There is a more economical and convincing way to deal with apparent inconsistencies than to say that respondents regularly make mistakes. The Socratic nature of Rawls's method can cut both ways. In addition to revising one's views of specific cases for consistency with abstract principles, one could revise the principles. The principle presented in the First Amendment is of course sweeping: "Congress shall make no law . . . abridging the freedom of speech." But the need for exceptions has long been recognized. In *On Liberty*, perhaps the most eloquent defense of free speech in our tradition, John Stuart Mill recognizes the need for exceptions to the general principle:

> Even opinions lose their immunity when the circumstances in which they are expressed are such as to constitute their expression a positive instigation to some mischievous act. An opinion that corn-dealers are starvers of the poor, or that private property is robbery, ought to be unmolested when simply circulated through the press, but may justly incur punishment when delivered orally to an excited mob assembled before the house of a corn-dealer, or when handed about among the same mob in the form of a placard." (1972, 114).

A similar principle is familiar in American jurisprudence. In the words of Justice Holmes, the right to free speech should be abridged only when a given speech would constitute a "clear and present danger" to society (*Schenck* v. *United States* 1919, 52). As I have noted, in "The Basic Liberties and Their Priority," Rawls surveys the history of constitutional doctrine in regard to free speech in order to demonstrate that despite apparent exceptions, it rests on a strong conception of rights.

Though I can only sketch my view here, I believe that the claim that Americans support a blanket principle of free speech (or any other strong democratic right) is overly simple. Rather, the evidence suggests that the principles Americans actually uphold contain numerous tacit exceptions. In certain cases, free speech can be viewed as destructive of public order, or the community's religious sensibilities. Confronted with such cases, many Americans will place regard for these other values before free speech. As McClosky and Brill argue, respect for rights is a single value that often exists in a context of other, competing values (1983, 431–34). Though individuals can easily express commitment to the value of freedom in the abstract (such support being cost-free), when they consider specific cases, they become aware of conflicting values and are willing to trade off between them.[15] Prothro and Grigg are inclined to see formal logical inconsistencies between adherence to abstract principles and departures from them in specific cases (1960, 292). However, because the moral principles to which Americans subscribe contain numerous tacit qualifications, they do not actually conflict with their views on specific cases. In forcing Americans carefully to consider what moral principles are most consistent with their considered judgments in difficult cases, Rawls's reflective equilibrium method would force them to realize that they intuitively qualify their moral principles. Though the sweeping claims presented in the Bill of Rights are in keeping with Rawls's strong conception of rights, these are more categorical than the political beliefs of most Americans, or citizens of other liberal democracies.

Striking confirmation of this line of argument is found in a recent study conducted by James Kuklinski and his colleagues, (1991) who examined the relative influences of cognition and affect on attitudes toward political tolerance. Briefly, the commonsense view, as they report it, is that intolerance is a visceral, emotional response to a group one dislikes and is capable of being controlled to some extent by moral principles (pp. 1–3). Their findings are at odds with this view. They asked their subjects to respond in different ways to standard questions measuring attitudes toward minority group rights. One group (the "affect group") was asked to respond according to its immediate, emotional reactions. A second group (the "consequence group") was asked to think carefully about the consequences of the relevant principles before responding. There was also a control ("no instruction") group that was not directed how to respond. Large majorities of all three groups supported civil liberties presented in the form of general principles. Interestingly, however, while the responses of the affect and no-instructions groups were similar and similar to those noted by other researchers, the consequence group's responses were notably *less* tolerant (pp. 8–13). The case is more complex in regard to specific applications of the general principles. Like previous researchers, Kuklinski and his colleagues note significant slippage when questions involve generally disliked groups, such as according rights to the Ku Klux Klan. Once again, the responses of the no-instructions and affect groups were similar, while the tolerant responses of the consequence group dropped dramatically (pp. 14–17).

In order to explain their findings, Kuklinski and his colleagues hypothesize that when people think about the consequences of general principles, their thoughts turn to adverse consequences and they express their fears (1991, 21–27). In other words, consideration of consequences calls attention to other values with which respect for rights can conflict. Kuklinski and his colleagues note that maintenance of democracy requires adherence to a complex set of values, only one of which is respect for other people's rights. An advantage of contemplation is that it causes people to consider a range of democratic values (p. 23). But the point remains that in practice—and, apparently, upon reflection—citizens strongly qualify general democratic principles to which they adhere.

## CRITIQUE OF RAWLS

If the political beliefs of Americans (and other liberal citizens) are as I have indicated, this has implications for both Rawls's specific claims about a strong conception of rights and his general view of overlapping consensus. I shall assume without argument that ideological factors contribute significantly to political stability and instability, though I will not attempt to assess the weight of such factors in comparison to others, especially underlying economic conditions. This is not an uncontroversial assumption, since, in the Marxian tradition especially, scholars view ideas as epiphenomenal, reflecting other societal factors and without independent influence. But conceding that ideas play an important independent role is not helpful to Rawls. It is not clear how stable democratic regimes are supported by the ideas I have discussed. If the political ideas of liberal citizens play an important role, we must explain how democracies can endure *in spite of* them, rather than because of them.

Underlying Rawls's "political" defense of his principles of justice appears to be what is referred to as the commonsense, or textbook, model of democratic stability. A staple of democratic theory for hundreds of years, this view is represented by Tocqueville, among others: "For society to exist and, even more, for society to prosper, it is essential that all the minds of the citizens should always be rallied and held together by some leading ideas" (1969, 434). A more recent proponent of the view is Roberta Sigel: "Political socialization refers to the learning process by which the political norms and behaviors acceptable to an ongoing political system are transmitted from generation to generation. . . . A well-functioning citizen is one who accepts (internalizes) society's political norms. . . . Without a body politic so in harmony with the ongoing political values, a political system would have trouble functioning smoothly" (quoted by Mann 1970, 423). But such a view has difficulty explaining away troubling empirical findings concerning what Americans believe.

The great emphasis Rawls places on overlapping consensus and how this differs from a modus vivendi leaves him on doubly shaky ground. His view combines the claim that democratic principles are central to political stability with the additional claim that the requisite principles must be not only maintained but held in a certain way, willingly adhered to in the belief that other people support them as well. Rawls also appears to believe that how principles are held plays a critical role in regime stability, apparently outweighing all other factors. There is little reason to believe that Rawls is right about these claims. His view obviously requires detailed defense, beyond the commonsense sociology on which he relies.[16] It is especially important for him to demonstrate that how principles are held is so significant, outweighing other sources of stability and instability that have been discussed by scholars since the time of Aristotle.[17] This is a serious weakness in Rawls's position, damaging to both his general political sociology and his specific defense of the principles of justice.

The evidence I have presented has serious implications for Rawls's political defense of the principles. His belief that citizens must uphold strong rights for the sake of stable democracies flies in the face of the fact that democracies have endured largely without the requisite values. It remains open for Rawls to say that the regimes we have discussed are not true "democracies," that the values he has in mind are necessary for truly *liberal* societies. A claim of this sort can be defended. Enduring and secure American democracy has proved compatible with frequent outbreaks of political repression of unpopular minorities. Notable targets have been citizens of Japanese ancestry during World War II and alleged communists or sympathizers during the McCarthy period, to say nothing of longstanding suppression of blacks in the South and elsewhere (Goldstein 1978). Recent surveys show high levels of intolerance against what are regarded as dangerous fringe groups at the present time (Gibson 1989b). But dismissal of American and other democracies as not sufficiently democratic does not square well with Rawls's political strategy. He could argue that strongly democratic societies are necessary to realize the values of existing citizens, but it is unlikely that this could be shown to be true of most actual citizens.

Rawls appears to be caught in a dilemma. He can defend democracy instrumentally, as necessary to the satisfaction of citizens' existing demands and, so, to an overlapping consensus. But the values of existing citizen would leave him with liberal principles that fall far short of his principles of justice. An attempt to defend more substantive principles would founder on the beliefs of actual citizens. To the extent that Rawls demands democratic regimes with substantially more normative content, his view would cease to rest on the shared views of liberal citizens and express, instead, the comprehensive view of one particular group. If the distinctive views of Mill or Kant are regarded as comprehensive and so not adequate foundations for an overlapping consensus (Rawls 1987, 5, 9; 1988, 267–68; 1989, 235), then a strong conception of democracy can be criticized along similar lines.[18] Rawls cannot have it both ways. The liberal principles that constitute an overlapping consensus must either be normatively thin or fall under an unacceptable comprehensive view. His contention that liberal political culture is based on "the fundamental intuitive idea of political society as a fair system of social cooperation between citizens regarded as free and equal persons" (1987, 7) appears to bridge the dilemma, but I believe it contains a tacit equivocation. If we take "free and equal persons" in a strong sense, it is part of a comprehensive view and so out of keeping with liberal political culture. If we construe it in broad, general terms, it will not support a liberal political theory with sufficient normative substance.

I conclude that Rawls's common sense sociology must be viewed as, at best, unproven and highly improbable. There is little reason to believe that consensus on specific beliefs, held in a specific way, is as important to political stability as he asserts. Though it is not impossible that these factors play a significant role, it is incumbent on Rawls to make this case. In addition, as we have seen, Rawls's consensus model of democratic stability is plagued by strong evidence indicating that the requisite consensus does not exist. I conclude that Rawls's political defense of the two principles fails on empirical grounds. A strong conception of rights does not appear to lie at the heart of liberal culture. Though aspects of our tradition reflect the requisite view of rights, others do not. If Rawls believes that the survey evidence we have reviewed is somehow incorrect, once again, it is incumbent on him to make this case. As things presently stand, the evidence weighs heavily against his position.

## AN ALTERNATIVE OVERLAPPING CONSENSUS

I shall now return to the question of how democracy can endure when citizens have the kinds of beliefs we have seen. I suggest that a rather different overlapping consensus can be identified, which does contribute to democratic stability, though it has less moral content than that propounded by Rawls. Although the empirical support for this claim is not developed, it bears mention here.

Scholars have advanced different hypotheses to explain how democratic values can endure in a society in which they are not held by large percentages of the population. One explanation centers on the fact that politically active members of the community—elites—appear to be markedly more tolerant than average citizens and that intolerant citizens generally take a less active part in politics (McClosky 1964; McClosky and Brill 1983).[19] Another argument emphasizes that intolerant citizens do not agree on targets and so do not easily combine in order to suppress particular groups (Sullivan, Piereson, and Marcus 1982). This line of argument is closely related to another that emphasizes American federalism (separation of powers and checks-and-balances), which deliberately makes it difficult to translate political sentiments, even those of majorities, into governmental policies.[20]

These contentions (and others, as well) are still debated by scholars; and I shall not venture to choose between them.[21] There is, however, little reason to view them as mutually exclusive, for several factors can combine in any one case. An additional factor appears to be important: if, along with Rawls, we attribute some significance to the role of value consensus, it seems that we should look for it in attitudes about the political system, rather than in more substantive moral principles.

There is empirical evidence that certain attitudes centering on trust in one's overall political system are bound up with the kind of behavior that a stable democracy requires. The specific attitude I have in mind is *diffuse political support*. Closely related to the notion of legitimacy, this is the feeling that the system can be counted on to produce outcomes that conform to one's moral principles. In his well-known study of political support, David Easton distinguishes diffuse support from "specific support." The latter centers on approval of specific policies, whereas the focus of the former is the political system as a whole. Easton describes it as "a reservoir of favorable attitudes or good will that helps members to accept or tolerate outputs to which they are opposed or the effect of which they see as damaging to their wants" (Easton 1965, 273; see also Easton 1975). The work of Edward N. Muller and his colleagues suggests two conclusions. First, high levels of diffuse support are correlated with low levels of "antisystem political behavior." Second, though evidence is not abundant, it appears that diffuse support in the United States and other liberal societies that have been examined is high.

Muller and his colleagues argue that there is a clear link between diffuse support and antisystem political behavior. This is defined as "political behavior that is illegal, disruptive of the normal functioning of government, and may entail the use of violence" (Muller and Jukam 1977; Muller, Jukam, and Seligson 1982).[22] Other things being equal, individuals who do not view the political system as legitimate are more likely to engage in illegal protests against it than people who support it as a whole, in spite of their low regard for specific policy outcomes: "By the most pedestrian logic," a low level of regime support "provides a source of normative justification for participation in extralegal protest and violence" (Muller, Jukam, and Seligson 1982, 245). Beyond a certain point, activity of this sort will bring down a given government. Such activity would prevent a political system from being an enduring and stable democracy of the kind Rawls wishes to foster.

Muller, Jukam, and Seligson (1982) measured diffuse support by constructing what they call a *political support–alienation* (PSA) scale, which measures various positive attitudes toward different aspects of the political system.[23] They surveyed respondents in West Germany, Guadalajara, Mexico, San Jose, Costa Rica, and New York City, all in the 1970s (see also Finkel, Muller, and Seligson 1989; Muller 1977). Their results show strong correlations between PSA scores and the propensity to engage in antisystem political behavior: "Individuals who register very low on the scale also report much higher levels of antisystem political behavior" (Miller, Jukam, and Seligson 1982, 253–63, esp. 263).[24]

The second point is that levels of diffuse support, as measured on Muller's scale, are quite high. Large majorities feel that the American political system is "the best possible," are proud to live under it, and feel that they should support it. In New York City, 63.6% of nonwhites and 72.2% of whites surveyed showed high support scores. A remarkable 88.4% of nonwhites demonstrate positive levels of support. The figures for whites was 91.9%. It is also notable that these scores emerged in spite of the low levels of trust in specific policies and incumbents that have showed up repeatedly in other surveys over the past 30 years.[25] In Costa Rica, the figures were even higher: 92.4% of respondents showed positive levels of support. In subsequent research, Finkel, Muller, and Seligson (1989) showed that this level of support in Costa Rica was only slightly affected by severe economic conditions.

Though one must be hesitant about drawing strong conclusions from this limited body of evidence—and generalizing about the United States as a whole from a single New York City sample—this is, unfortunately, the best evidence we have concerning political support–alienation and, so, diffuse political support. This evidence suggests that if ideas are, indeed, a significant factor in political stability, the political stability of American democracy can be attributed, in part, to the large majority's belief in the legitimacy of the political system.[26]

In closing, then, the best evidence we have suggests the existence of what Curt Baier has called a "constitutional consensus" in liberal democracy (1989, 775). Americans, by and large, are willing to accept the outcomes of their political process despite a lack of agreement on substantive moral principles of the kind that Rawls has in mind. The survey research we have examined suggests that scholars who seek an actual overlapping consensus should focus on this.[27]

Not surprisingly, this is a prominent theme in classic works of our political tradition. As Locke argues, when a society is widely believed to have political procedures that are arbitrary and unjust, the population is not unlikely to rise (*Second Treatise of Government* 225). We can amend this: when a significant percentage of the population loses faith in the acceptability of political procedures, they are more likely to engage in antisystem behavior and so threaten political stability.

Although the American political system violates a strong view of rights in repressing unpopular groups, in doing so it does not appear to violate public opinion. Given the attitudes of the American population, the occasional repression of unpopular minorities seems difficult to avoid. However, as long as the system as a whole continues to be viewed in a favorable light by a large majority of the population, American and other liberal democracies can continue to be stable and endure.

## Notes

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1. We can distinguish three separate stages in his defense of the principles: (1) "Justice as Fairness" (1958), given an excellent critique in Wolff (1977, chap. 5); (2) *A Theory of Justice* (1971); (3) the articles discussed herein. I will not discuss the possibility that "Kantian Constructivism in Moral Theory" (1980) and "The Basic Liberties and Their Priority" (1982) represent a distinct (fourth) stage in Rawls's development, though this claim could be defended. Clearly, seeds of the political argument are present in them, and I will draw on them in that connection.

2. This is in opposition to the recent argument of Paris (1991), which questions the possibility of an overlapping consensus based on broad moral principles. See n. 27.

3. His consistent opposition to utilitarianism is apparent throughout Rawls 1958, as well as Rawls 1971 (esp. pp. 3–6, 22–33, 150–92). See also Rawls 1985, 226; 1987, 19–20.

4. We find similar presentations of facts in Rawls 1985, 1987, and 1989. This material is not discussed directly in Rawls 1988, but it is presupposed. A similar list of facts is presented in Rawls 1987, 4, n. 7. I assume that the factual statements are consistent from article to article; slight discrepancies should not affect my argument.

5. Rawls does not consistently supply percentages, that is, whether he means majority, large majority, or near unanimity. The case is similar in regard to who is included: citizens, politically active citizens, or all inhabitants. My usage throughout this paper is accordingly rough, to reflect these uncertainties.

6. Valuable discussions and critiques of Rawls's recent position include Baier 1989; Galston 1991, chap. 7; Hampton 1989; and Kukathas and Pettit 1990, chap. 7.

7. Lipset defines "democracy" in regard to institutions that provide means peacefully to change governing officials (1981, 27–28). A major movement is defined as one receiving at least 20% of the vote in an election during the period noted (p. 30, n. 7). Lipset uses somewhat less demanding criteria for less stable Latin American democracies (pp. 30–31).

8. For Rawls's distinction between "stable" and "secure" regimes, see n. 17.

9. I will not reproduce the two principles here. For a recent formulation, see Rawls 1989, 251, n. 43; cf. 1971 60, 302.

10. Although Rawls says that justice as fairness "tries to draw *solely* upon basic intuitive ideas that are embedded in" liberal political culture (1985, 225; emphasis mine), he also presents an argument for his principles that relies on a two-stage sequence (1989, esp. 234, 250–51). Briefly, Rawls argues that in the first stage, justice as fairness should be viewed as a "free-standing conception" derived from the original position as presented in *A Theory of Justice*, or the like (1989, 250–51). In the second stage, the political argument kicks in, and Rawls inquires whether the principles derived in the first stage can ground an overlapping consensus that is able to generate its own support. It is clear that to the extent that Rawls takes the political argument seriously, the two-stage view cannot circumvent the empirical problems I have discussed. If "a political understanding of constitutional essentials is of utmost urgency" in securing social cooperation (p. 241), then the contents of the political view must be congenial to liberal culture, regardless where the principles originally came from.

Two additional points bear noting. First, it appears that Rawls is confident that there is no conflict between the two-stage model and his political argument. Not only is the political argument the core of the second stage, but Rawls apparently believes that the principles generated in the first stage will fit with liberal culture (1987, 7, n. 13). Second, I do not discuss the position that he advances throughout Rawls 1980 (and, to a lesser extent, in later works), namely, that suitable principles of justice must be derived from specific intuitive ideas, to wit, the liberal conception of the person and of society as based on free cooperation. This procedure also fails to support the political argument, unless there is the requisite fit between resultant conceptions and liberal culture.

11. Although I shall generally concentrate on the United States here, similar evidence is available for other liberal societies (see Sullivan et al. 1985). For the views of Britons, see Barnum and Sullivan 1989, 1990 and Budge 1970.

12. For major theoretical issues, see, esp., Sullivan, Piereson, and Marcus 1979, 1982. Two recent criticisms of their view are Sniderman et al. 1989; and Gibson 1989b. Other important studies include McClosky 1964 and McClosky and Brill 1983. Additional references are found below.

13. For the theory behind asking respondents about their "least-liked" groups, see Sullivan, Piereson, and Marcus 1979, 1982; see also the criticisms cited in n. 12.

14. Prothro and Grigg note more tolerant attitudes than does Stouffer in regard to certain questions and believe that the differences can be attributed largely to the superior level of education in the academic communities where they conducted their survey. Response levels for the members of Prothro and Grigg's sample with low education were closer to those of Stouffer. For discussion of the connection between education and tolerance, see McClosky and Brill 1983, chap. 6; Prothro and Grigg 1960, 288; Sullivan, Piereson, and Marcus 1982, 114–26. See also McClosky and Zaller 1984, chap. 1.

15. Cf. McClosky and Brill 1983, 422–32. Note their discussion of connections between views on toleration and political ideology, especially the views of political conservatives (chap. 7 and pp. 422–23). A somewhat different view that should be noted is that of Mann (1970), who argues that adherence to the general principles along with the conflicting applications is the result of childhood socialization and, so, is "false consciousness."

16. According to Baier, Rawls's analysis could be called "armchair sociology or political science" (1989, 783).

17. Rawls's belief in the importance of moral principles' generating their own support shows up in the distinction between "stable" and "secure" societies (1989, 234–35, 239), as well as that between overlapping consensus and modus vivendi. See also Rawls 1971, secs. 29, 69.

18. It should be noted that as things stand, certain scholars criticize Rawls's view for, in effect, being comprehensive (e.g., Galston 1989; Macedo 1990).

19. Two important recent discussions are Gibson 1988 and 1989a. It should be noted that an elite theory of democracy is potentially of interest to students of Rawls. According to this view, the democratic creed is borne by elites (variously defined), rather than society as a whole. Repeated studies have shown that elites are more tolerant than average citizens. If this is true, then, conceivably, the overlapping consensus that is actually required for a stable democracy could be found among this stratum of society.

20. The classic exposition is *Federalist* 10; see also Lipset 1981, chap. 2; Sullivan, Piereson, and Marcus note the similarities between their view and this (1982, 256).

21. For problems in measuring and assessing the influence of different dimensions of political support, see Weatherford 1992 and Wright 1981.

22. The quoted words are Muller and Jukam's definition of "aggressive political behavior" (1977, 1573). Although there are slight improvements in the survey instruments used between 1977 and 1982, the concepts can be regarded as synonymous (see Muller, Jukam, and Seligson 1982, 246–47).

23. The survey questions used by Muller, Jukam, and Seligson are as follows: (A) To what extent do you have respect for the political institutions in [COUNTRY]? (B) To what extent do you think that the courts in [COUNTRY] guarantee a

fair trial? (C) To what extent do you feel that the basic rights of citizens are well protected by our political system? (D) To what extent are you proud to live under our political system? (E) To what extent do you feel our system of government is the best possible system? (F) To what extent do you feel you should support our system of government? (G) To what extent do you feel you and your friends are well represented in our political system? (H) To what extent do you feel that your own political values differ from those of our political system? (1982, 249).

24. An important conclusion of their article is that this correlation is far stronger than that between antisystem political behavior and low levels of "trust" in government. Figures for trust in government, from 1958 to 1988, are conveniently available in Erikson, Luttbeg, and Tedin 1991, 121. For controversy over the interpretation of these figures, see Citrin 1974; Miller 1974a, 1974b.

25. See n. 24.

26. This conclusion is supported by extensive research on attitudes toward procedural justice. Repeated studies have shown that respondents are more willing to accept specific unfavorable decisions if they view the procedures through which they are made as "fair." See Lind and Tyler 1988 and Tyler 1990. For criteria used in assessing the fairness of procedures and evidence of agreement on criteria across demographic categories, see Tyler 1988.

27. This suggestion ties in with familiar accounts of liberal political theory, as (for instance) that of Benn and Peters (1959). It could perhaps be objected that Rawls might not regard diffuse political support as a suitable basis for an overlapping consensus, which is an agreement on *principles* to which one can appeal in deliberations concerning public policy. This is a difficult question on which I can only touch here, avoiding the complex issue of what constitutes a moral principle. It seems clear, however, that a general requirement to be "concluded by the majority" (Locke, *Second Treatise of Government* 96) is a moral commitment to which one can appeal in dialogues on public policy in order to counter opposed demands and so help to stabilize a society. Nor are decisions of policy processes viewed as binding without condition. For the limits within which political processes should be viewed as acceptable and so binding, see Klosko 1992, chap. 3. For a more detailed discussion (with numerous references) of the "reflective equilibrium" method according to which such determinations should be made, see chap. 1, sec. 4. For evidence of widespread agreement about criteria that are used to assess the fairness of decision mechanisms, see pp. 67–72, as well as Tyler 1988. Once again, there is nothing in my discussion to rule out the contributions to stability of other factors discussed in the literature, for example, those discussed by Lipset (1981) or Paris (1991). It also bears mention that diffuse political support is not deeply abstract and philosophical and, so, is less subject to the kinds of disagreement over interpretation that Paris notes in regard to Rawlsian principles (1991, esp. 893–94).

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# THE POLITICS OF VIRTUE TODAY: A CRITIQUE AND A PROPOSAL

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*I argue for a distinction between publicly oriented and privately oriented conceptions of civic virtue. I first provide a critique of two current politics of virtue (liberal and republican), arguing (1) that despite their differences, they are both publicly oriented and (2) that their problems lie in this public orientation. I conclude by arguing for the legitimacy and promise of a privately oriented politics of civic virtue, using as examples of this approach* Cato's Letters *from the early eighteenth century and the work of two contemporary theorists, Bruce Ackerman and Stephen Elkin.*

In recent years, political philosophers across the ideological spectrum have rediscovered virtue. In fact, the idea of reinvigorating public life with suitable doses of citizenly devotion has proved so compelling to theorists in recent years that we can identify a range of politics of virtue on offer today, each with its own problems and possibilities.

Proponents of an Aristotelian politics of virtue want to make the cultivation of personal and public virtues the primary end of political life. They argue that modern governments can and should follow the example of Aristotle's best constitution and give "first place to considerations of excellence of character and how it may be cultivated" (Budziszewski 1988, 17). For these neo-Aristotelians, character development is the province not only of Mom, Dad, and the Boy Scouts, but of legislatures and the courts. Nurturing a sense of "rational purposefulness" or "rational self-understanding" in the citizenry and politically rewarding its presence can and should be the primary goal of all good polities, including liberal democratic ones (Budziszewski 1988, 17, 11).[1]

When it frames the proposed tasks of government with appropriate caution—informed, perhaps, by a liberal skepticism toward political authority—the Aristotelian perspective on virtue can offer a compelling vision of public life. Certainly, advocates of such a politics make a convincing case that some sustained attention to the virtues of the citizen body is both possible and desirable today. Still, to find the Aristotelian approach completely satisfying, one must believe that the end of politics in contemporary constitutional democracies ought to be, or inevitably is, the cultivation of virtue. I want to focus here on an alternate and at least equally influential approach to citizen virtue, one that works within a more conventional understanding of the ends of politics.

We might call this species of virtue politics *instrumental* in that the development of virtuous citizens is not seen as the end or purpose of political life. Rather, the political or civic virtues are praised for being especially good means to the advancement of other worthy political ends, such as liberty, equality, democracy, and order. Thus, communitarians and other participants in the republican revival tend to identify as civic virtues those qualities that would make private individuals more citizenly, involving them more actively in a democratic polity. Liberals willing to broach the topic of virtue at all urge the cultivation or appreciation of character traits that would make citizens better liberals (e.g., personal autonomy, tolerance). While the dispositions recommended in these contexts generally represent certain moral excellences, they are not valued or pursued for this reason. Rather, they are conceived, in more instrumental terms, as those dispositions or characteristics of the person that best fit individuals to contribute to the flourishing of their political community.

Typically, those who have attempted to assess the claims made by this more instrumental politics of virtue have proceeded on two fronts: they debate the relative merits of a particular theorist's conception of civic virtue or argue about which vision of the good citizen is more attractive—the liberals' or republicans'. Such criticism, however, while valuable, fails to go far enough and does not take seriously the *politics* of the politics of virtue. Both the liberal and republican politics of virtue offered today at least implicitly urge a particular course of political action, namely, the cultivation of the desired virtues or character traits in the contemporary citizenry. Any evaluation of the modern politics of virtue must respect these real-world aspirations and judge competing conceptions of political virtue at least partially on whether they make proper allowances for the nature, habits, and spirit of the people they hope to reform.

This standard does not mean rank-ordering political theories according to the chances of their visions' being realized. On the other hand, proposals for the renewal of political life that presuppose resources, capabilities, and political will that until now have been completely absent from any modern polity may properly be criticized for their utopian quality. I suggest a new set of categories by which to judge the politics of virtue, one which moves beyond the standard liberal/republican distinction to a new view of the varieties of political virtue and their relative potentials for political success in contemporary constitutional democracies.[2]

## THE VARIETIES OF VIRTUE

### Public and Private

For certain modern commentators, the term *civic virtue* stands simply for "the moral and political qualities that make a good citizen," in whatever regime the citizen may be found (Walzer 1980, 55; see also Sinopoli 1987, 344). This conflation of civic and political virtue has the advantage of simplicity yet slights the particularly charged quality that the former term has acquired in recent years. To say that individuals have *civic* virtue seems to say something very definite about the regime in which these virtuous individuals live, the qualities they possess, and the activities in the public realm that these qualities support or to which they dispose. For this reason, I treat civic virtue as a specific sort of political virtue, one particularly appropriate to, and manifested in, a political regime that allows for a civic mode of life. This civic, as opposed to merely political, mode of life is one grounded in "participatory self-rule." It is bounded on one side by despotism and on the other by extreme forms of "procedural liberalism" (Taylor 1989, 178, 172). Between these poles lie a variety of regimes in which individuals participate, to a greater or lesser extent, in the shaping of their collective destiny.

The accounts of civic virtue with which we are most familiar posit a stark divide between public and private. Within the republican tradition, especially, the qualities praised as virtuous are those that enable or dispose the citizen to privilege the public, political realm over private, personal desires and ambitions.[3] Thus, Carter Braxton, writing during the American Revolution, defines public virtue as "a disinterested attachment to the public good, exclusive and independent of all private and selfish interest" (cited in Wood 1969, 96). Historians of political thought echo this terminology: "For most classical republicans, liberty could only be achieved by each man's willingness to renounce his purely private concerns for the greater good of the community" (Pagden 1987, 10; see also Kramnick 1988, 15; Wood 1969, 68). In all these examples, the qualities that make a citizen virtuous, while variously described, hinge on a mindset in which the goods of the public realm, the world of political action and deliberation, are given priority over private goods—whether from a rational decision to set aside "private interests" or from an intense emotional engagement with the public and its goods (liberty, national honor, political action itself).[4] I call conceptions of political or civic virtue grounded in such a mindset *publicly oriented*.

There is however no necessary identity between the qualities that advance the flourishing of the state and safeguard political liberty and the sort of aggressively public-minded self-sacrificing behavior praised by representatives of the republican tradition. Public spirit, patriotism, love of country, love of equality, and the willingness to subordinate private to public good are simply the particular answers given consistently throughout the republican tradition to the question, What citizen excellences are necessary to preserve a free and flourishing society? But it is at least conceivable that individuals would be able to serve a civic regime without possessing the passionate attachment to the polity and its needs that grounds the more familiar sort of publicly oriented civic virtue just described. This is not to say that such individuals would suddenly have become "perfect privatists," inhabitants of a liberal state that asks nothing from them but that they treat others and others' life goals with equal concern and respect.[5] Rather, their civic virtues would rest on a different basis from the enthusiastic or self-sacrificing "public spirit" characteristic of the republican tradition. Civic virtues of this sort I call *privately oriented*.

Keeping this distinction between publicly and privately oriented virtue in mind, we can now consider more closely the various contemporary liberal and republican politics of virtue on offer today. I want to show both that these politics endorse a publicly oriented form of civic virtue and that this public orientation creates problems for both the possibility and desirability of establishing a politics of virtue today.

### Liberal and Republican

Contemporary theorists proposing a republican politics of virtue usually characterize the good citizen as the good deliberator, someone who participates eagerly and competently in the conversations that are central to twentieth-century republicans' understanding of good political life. Interestingly, what makes a good deliberator for these contemporary republicans is not the sort of aggressively public-minded, self-sacrificing behavior praised by earlier representatives of the republican tradition. Rather, the requisite quality is "critical distance from prevailing desires and practices" (Sunstein 1988, 1549), or, the "capacity for reflexively critical reconsideration of [one's] ends and commitments" (Michelman 1988, 1528). To deliberate well about common ends—to exercise republican civic virtue—requires some degree of detachment from self, the subordination of personal interest to the practice of dialogue.

The growing number of liberal theorists prepared to endorse a politics of virtue describe the end of the good polity and the citizen virtues that advance it in a somewhat different way. Stephen Macedo in his book *Liberal Virtues* argues that the best liberal regime endorses a "situated autonomy," grounded in the free individual's "critical capacity to interpret and shape nature and desire" (Macedo 1990, 213). Rogers Smith places a similarly demanding conception of individual liberty at the center of the liberal state. A liberal polity, he argues, dedicates itself to "what has always been the deepest concern of liberal thought, the promotion of personal capacities for reflective self-direction" (1985, 171).

These passages suggest that liberals, as well as republicans, consider the ability to distance oneself from one's immediate preferences as crucial to good citizenship. But while republican citizens place their critical abilities in the service of an expansive and directed dialogue with their fellow countrymen, good liberal citizens (as befits the liberal satisfaction with representative democracy) are primarily good judges—of the actions of their representatives, of the achievements of their government, of the state of their liberties. Smith, for example, envisions the good citizen as a sort of public ombudsman continually assessing how he or she, as well as society at large, measures up to the goals set by the ideal of rational liberty. Good citizens must, first of all, look inward, cultivating liberal dispositions on a personal level. They must then look outward, judging, urging, and prodding their government to a better realization of liberal ideals: "Particularly when they act as collective deliberative self-governors, through democratic political processes, such liberal citizens should ask whether basic economic, educational and cultural arrangements are working adequately to empower all, or whether some are being systematically neglected or constrained. When such oppressive political and social institutions are found, a liberal polity must seek their reconstruction" (Smith 1985, 291). Macedo too, identifies the demeanor of the good citizen as one of "critical reflectiveness on public principles," writing that "liberal citizens are called upon to take up the attitude of the ideal judge, Hercules, and to act in politics as critical interpreters of public moral principles" (1990, 128, 102).[6]

Both liberals and republicans then offer an account of civic virtue for our consideration, a description of the dispositions needed to keep a regime free and flourishing. Liberals want citizens who judge well, republicans, ones who deliberate well. Despite the difference in context and evidence, these various politics of virtue ground a citizen's excellence—performance of praiseworthy political deeds—in an emotional engagement with the polity and its principles. Whether the mark of civic virtue is the ability to deliberate well or to judge perceptively, the anterior requirement is to embrace, in thought and deed, the public world, to engage in politics for abstractly public reasons.

Thus, Benjamin Barber, after identifying "civility" as the characteristic civic virtue of good democrats, admits that his politics requires "an infusion of communitarian values." Loyalty, fraternity, patriotism, neighborliness, bonding, tradition, mutual affection, and common belief are all necessary to make his strong democrats willing—indeed, eager—to attend neighborhood political assemblies, to inform themselves and vote in national referenda, to explore the content of the country's common good. So the good deliberator must, in fact, possess a very public civic virtue, that is, devotion to the participatory politics that underlies the republican vision of political community (Barber 1984, 243).

The liberal politics of virtue also envisions a virtue grounded in an emotional commitment to the public realm, a passionate attachment to public goods and practices. Smith argues that "a liberal polity should be united . . . by a shared political and social purpose: to promote ways of life that advance liberty for all" (Smith 1989, 290). Macedo, too, calls for "citizens . . . united by their devotion to a public morality," individuals willing to affirm the ideal of liberal justice "as a supreme moral commitment" (Macedo 1990, 273, 255). Good liberals, then, like good republicans, act out of commitment to a very public purpose—in this case, a demanding philosophy of liberty, a desire "to promote ways of life that advance liberty for all," an end they seek not out of any Tocquevillean "enlightened self-interest" but because it is right and good for the community as a whole, since for virtuous citizens, furthering the "commitment [to rational liberty] is what their civic life is really all about" (Smith 1989, 290).

These publicly oriented conceptions of civic virtue receive an eloquent defense in the various works already discussed. But how plausible are any of these accounts when considered as real political proposals? Would an effort actually to cultivate these specific qualities in the modern citizen, to create the political and social institutions needed to nurture virtuous citizens be possible or desirable?

In fact, neither liberal nor republican versions of the politics of virtue hold much promise for engaging the political enthusiasms of the American people or of successfully negotiating the democratic political process. This is not to deny that each presents a challenging and at times compelling vision of what, in ideal circumstances, good citizens of the modern polity might look like. But despite what first appearances might suggest, the theories discussed all advance robustly *public* conceptions of the good citizen and, as such, are subject to a number of difficulties that recommend another sort of civic virtue to our attention.

## THE PROBLEM WITH PUBLIC VIRTUE

The precariousness of public virtue in the contemporary world has been observed and lamented before. But I shall propose a different explanation for such precariousness from those usually offered. Those who doubt the possibility of a genuinely civic virtue today often blame the emergence of liberal, commercial society for making a robust involvement with, or dedication to, public ends unlikely or impossible. But whatever the impact of liberal ideals or a market economy on individuals' sense of themselves and their priorities, it is still a mistake to blame the dominance of liberal ideology or the spread of commercial enterprise for placing a politics of public virtue beyond the reach of twentieth-century constitutional democracies.

In this context, it is important to recall just how skeptical the great republican theorists were of the possibility of establishing and maintaining a regime favorable to virtue. Both Machiavelli and Rousseau, for example, insisted that virtuous citizens were products of a virtuous founding and that a virtuous founding required a severe legislator imbued with divine authority who shaped a regime's constitution so as to bind citizens emotionally and materially to the republic. Any such achievement was seen as open to corruption, and both Machiavelli and Rousseau doubted the possibility, except in the most extreme circumstances, of returning a corrupt citizenry to the practice of public virtue.[7] These philosophers, then, did not tie the uncertain prospects for citizen virtue to the emergence of liberal theories of political obligation or of a modern system of commerce and finance. Rather their concern was with certain enduring human passions—self-love, ambition, desire for material comfort—which present a persistent challenge to the realization of any politics of civic virtue, a challenge the contemporary calls for a renewal of civic virtue have ignored almost completely.

To put this point in a different way, given men and women as they are, it is difficult to conceive of the circumstances under which a Western democracy would embrace the changes in popular culture, lifestyle, and political process that would be necessary to make either the liberal or republican version of the politics of virtue a reality. Some may wish to blame our liberal heritage or the commercial ethos for stripping individuals of the potential to be truly civic beings. It seems to me, drawing on the insights of Machiavelli and Rousseau, that the problem is more profound, linked not to particularly modern conditions but to human desires and dispositions with which all political communities have struggled. Public virtue of any sort asks too great an abstraction from self and the parochial interests that conventionally draw citizens into politics to have any chance of being successfully cultivated in a community that must establish its conditions through the legislative process. (The alternative route, contemplated by the great republican thinkers, is to have the sources of virtue imposed from above at the founding of a republic, not an open option at the present time). Public virtue, then, is a victim not of liberalism or commerce but of politics and the necessary conditions of democratic public life.

## Republican Failings

Barber's admirably detailed account of the institutional reforms necessary to transform the United States into a "strong democracy" provides a good example of the failure to think through the *politics* of a politics of public virtue. Central to Barber's "systematic program of participatory reform" is a "national system of *neighborhood assemblies* in every rural, suburban, and urban district in America" (Barber 1984, 269). These assemblies would meet weekly, encouraging the development of "civic competence" from "the discussion of a flexible and citizen-generated agenda" (pp. 264–65, 270). Were such assemblies established, I am quite prepared to grant Barber the civic benefits he attributes to them. But what are the odds of realizing such an achievement? To imagine the circumstances in which a reasonably powerful coalition of public activists and community groups would (1) endorse such a proposal and (2) convince Congress to fund it is difficult enough. But imagining the conditions under which a significant number of American citizens would free up their Saturday afternoons or Wednesday evenings (Barber's scheduling suggestions) so that they might "examine different legislative positions in detail, assess the local impact of regional and national bills, explore ideological stances in the absence of pressures from special-interest groups, and introduce new questions of interest to the neighborhood" (p. 271) is impossible.[8]

These objections are not meant to condemn Americans as beyond civic virtue, unable to muster the dispositions necessary to contribute positively to the flourishing of a democratic state. But they are meant to suggest that a politics of virtue that requires of the good citizen a willingness to spend long hours debating matters of public concern "in which [citizens'] most immediate interests would be ignored" (Barber 1984, 265) is destined to fail. I use such strongly determinist language deliberately. Such proposals place far too great a demand on an individual's time and emotional resources to garner much support in any deliberative democratic process. (As a working parent of young children, I call these reservations "the babysitter problem" and the "me-first factor," respectively.)

My critique is not directed against the workability of communitarian proposals per se. Individuals born and raised to civic maturity in a polity characterized by Barber's strong democratic institutions would learn to live with, and perhaps love, such neighborhood assemblies or other trappings of a republican politics. It is not that human beings are incapable of living within strongly participatory political cultures. The question, however, is how one gets there from here without some form of nondemocratic founding moment. By requiring of good citizens a passionate and time-consuming involvement in the public realm, republicans and communitarians articulate a vision of civic virtue that necessarily excludes the hard-working and often undereducated citizens of any truly inclusive democratic polity.[9]

The liberal politics of virtue just surveyed do not call for the participatory virtues envisioned by republicans and thus avoid political difficulty on this account. However, the United States is equally unlikely to produce a race of citizens that make commitment to a liberal public philosophy their primary motive for political action and involvement. To demand such a devotion to liberal justice or rational liberty as the mark of civic virtue sets a standard far beyond the reach of most American citizens, even those with largely liberal sympathies.

Liberal politics that insist on good citizens' embracing a publicly oriented civic virtue run the risk of forgetting one of the most compelling insights of liberal philosophy: what brings us into politics in the first place is the desire to satisfy or protect our personal interests. Indeed, our satisfaction with our leaders and our government is often a function of how well we perceive them to be meeting our interests. Any effort to wean citizens away from an attachment to the commonwealth based on these personal considerations seems not only misguided but unlikely to succeed. What possible plan do liberal theorists possess for convincing citizens that they ought to judge their government not on whether it makes their life better but on whether it fosters particularly liberal sorts of excellences in them and their neighbors?

A democratic or liberal politics of virtue that transcends self-interest is a seductive but illusory ideal. The elusive nature of this option derives not from some fundamental corruption or constitutional idiosyncracy afflicting modern culture and spared the ancients. The problem dogging the politics of virtue today is the one with which all seekers after civic virtue have had to contend. Tocqueville, for one, understood the problem well:

> It is difficult to draw a man out of his own circle to interest him in the destiny of the state, because he does not clearly understand what influence the destiny of the state can have upon his own lot. But if it is proposed to make a road cross the end of his estate, he will see at a glance that there is a connection between this small public affair and his greatest private affairs; and he will discover, without its being shown to him, the close tie that unites private to general interest. (1945, vol. 2, p. 111).

Rousseau is another reliable guide to the necessary limits of virtue. The apostle of the general will, he still believed that "one can make men act only by appealing to their self-interest" (1972, 70). Neither Rousseau nor Tocqueville blame liberal ideology or commercial society for the difficulties faced by those who would attach citizens' sentiments more or less exclusively to public ideals. Rather, they recognize that, whatever a polity's constitutional ideals, it will be difficult to persuade citizens to embrace and work to further them from the nobility or justice of the ideals themselves. It can perhaps be done: Rousseau's political writings set out what, for him, were the necessary steps. But a politics of public virtue (whether liberal or republican) that calls on citizens to forsake the private for the public without providing for the sort of political institutions and procedures recommended by classical republican theorists cannot succeed.

## CIVIC VIRTUES WE CAN LIVE WITH

Is the caricature of modern liberal democracy, then, correct after all? Must we abandon civic virtue for the mere manipulation or accommodation of self-interest? This conclusion is too extreme. It fails to recognize the possibility of a civic virtue that is not publicly oriented. By distinguishing privately from publicly oriented civic virtue as I have proposed, we can recognize and encourage a form of civic virtue well within the reach of contemporary citizens.

### An Eighteenth-Century Exemplar

To set out more concretely the nature and necessary preconditions of the sort of privately oriented civic virtue I have in mind, I want to introduce the account of citizen virtue argued for in *Cato's Letters*, an influential collection of essays penned in England in the 1720s by two Whig opponents to Walpole's ministry, John Trenchard and Thomas Gordon.[10] The good citizen of *Cato's Letters* very much resembles the good judge of the modern liberal vision. He keeps vigilant watch on the behavior of elected officials, inspects public policies for any unjust or tyrannical designs, and protests government activity that harms the public good. However, the motivation for this civic service is not an abstract belief or commitment to the ideals of a liberal polity but a very concrete sense of the benefits to which each individual is entitled in a free society. Government, says Cato, should further the public interest; and the best judge of its success or failure in this end is the citizens' own experience: "The People's Interest is the Public Interest; it signifies the same thing" (1.106). By "people," Cato has in mind what we might call all potentially active citizens. In Cato's day, this class included not only relatively well off enfranchised males but a larger group of men and women whose political grievances might mobilize them to extraconstitutional forms of protest. What I want to emphasize about Cato's account is that it casts self-interest not as an obstacle to civic virtue but as the source of individuals' positive contributions to the public good.

A historical example might help clarify what Cato has in mind. Imagine a disastrous government-sponsored financial scheme in which many small investors are ruined and several prominent government officials implicated. (*Cato's Letters* debuted in the aftermath of just such a scandal, the South Sea Bubble.) Cato calls on his countrymen to demand the public investigation and punishment of those responsible for the debacle regardless of political position. On what basis might individuals be moved to serve the public in this way? One might act to rid government of corrupt speculators out of a sense of public duty or love of country, but this notion of what motivates the virtuous citizen completely misses the point of what brings individuals to political action in the first place. In this example, Cato expects those with a *personal* interest in seeing justice done to be the most motivated and the most effective in advancing the public good—precisely because what satisfies their desire for personal vengeance on a set of corrupt officials also satisfies the public's need to get the same men out of office and in jail. Thus, Cato contends that especially in times of crisis or government malfeasance, ordinary citizens can be counted on to serve the public simply by following their self-interest: "The Whole People, by consulting their own Interest, consult the Publick, and act for the Public by acting for themselves" (*Cato's Letters* 2.41).

One might think also in this context of the extraordinary civic valor of the peoples of Eastern Europe and the former Soviet Union in creating the conditions under which regimes of participatory self-rule became at least thinkable. Their civic virtues were born of intensely personal desires for cultural particularity, freedom from communist rule, and material comfort and convenience. Looking to further (not subordinate) their private ends, they acted with great political courage to create free, self-governing communities, as well. Theirs was, in my sense, a privately oriented civic virtue.

One cannot, however, create virtuous citizens of the sort Cato praises simply by declaring that the public interest and the private interest signify the same thing. For individuals to manifest the sort of civic virtue that Cato recommends, there must first exist a genuine fit between the public good and citizens' private interest. One reason that classical republicans emphasized overcoming or subordinating private interest in their accounts of civic virtue is that historically, they addressed themselves primarily to an ambitious elite whose private interests (a desire for power and prestige) were, indeed, at odds with the republicans' hope for a free polity. One of the strengths, then, of a democratic polity of either the liberal or republican variety is that it expands the political realm to include the vast majority of a nation's inhabitants for whom the fit between personal and public good is tighter than it is with an elite bent on political domination. Ordinary men and women can, in Cato's vision, be counted on to keep a democratic government faithful to its constitutional principles and just and efficient in its operations—not because they harbor any profound loyalty to a public morality or political ideal but because a well-functioning democratic government is most responsive to their personal desires for liberty, security, and material comfort.[11] In such polities, Cato's willingness to ground civic virtue in the citizens' pursuit of their self-interest makes good sense.

### Grounding the Good Citizen

Civic virtue of the sort Cato has in mind also requires individuals who understand themselves as citizens and take seriously the rights and responsibilities that this role implies. When called upon to participate in public political deliberation, they must be able to put aside self-interest narrowly conceived (in which the horizons of interest stop at one's front doorstep) and consider, instead, what is in their self-interest as members of the political community. Not only must they possess an understanding of their polity's constitution (so as to judge when and whether its principles are being violated), but they must understand themselves as possessing rights and privileges worth defending through political action.

To secure this self-understanding requires a considerable amount of explicit and implicit political education, but this education neither makes any unusual demands on the basic impulses of human nature (the scope of self-interest is to be expanded, not narrowed) nor would require an extraordinary commitment of resources by public institutions. In fact, the various institutions through which this education for citizen virtue could take place are already in place in the modern democratic polity. The media, the schools, voluntary associations, local political organizations, family, and church can all reinforce the understanding of self as a bearer of political rights that deserve realization.

Once this self-understanding is achieved, it can provide a powerful impetus to civically virtuous political action. Both the Woman Suffrage and Civil Rights movements provide examples of citizens acting to further both their own interest (inclusion in the political process) and the public good (the achievement of a truly inclusive civic regime). The best possible citizen of a civic regime would also be driven to assure that *all* individuals had their rights respected and justice done to them. But, because Cato does not envision the ethnic, racial, and religious cleavages that challenge modern democracies, he is willing to rely on a "bootstrap" vision of civic virtue in which each citizen, defending his or her own interests and rights, secures the interests of the polity, as well. An expansive concern for the rights of minorities is not strictly necessary to Cato's vision of virtue.

Here, then, is the beginning of a viable account of civic virtue that is privately, rather than publicly, oriented. Cato's citizens defend and sustain the public good of a political community committed to some form of participatory self-rule (thus, they are civically virtuous), but they act primarily out of concern for their personal welfare (thus, the virtue is privately oriented). *Cato's Letters* does not necessarily provide a prescription for civic virtue in the modern polity, but it does suggest a model on which a contemporary politics of virtue might build.

## AMERICAN VIRTUES: TWO POSSIBILITIES

One shortcoming of Cato's particular account of privately oriented civic virtue is its limited sense of the political activities appropriate to the good citizen. The citizen's role remains primarily that of watchdog—to react to danger, to alert the community to malfeasance and pressure the government for its correction. To the extent that we aspire, as well, to a polity in which citizens shape and reshape the political agenda—in which they decide what ends the government should pursue, as well as judge how well it pursues them—more is needed. As it happens, at least two recent works in political philosophy have advanced suggestions for a modern politics of civic virtue that might make good the shortcomings in Cato's analysis while continuing to offer to twentieth-century democrats an attractive and achievable, privately oriented vision of the virtuous citizen.

### Elkin's Self-Interested Citizen

Stephen Elkin, in *City and Regime in the American Republic* (1987), proposes a restructuring of city government so as to nurture a citizenry better capable of deliberating upon what he calls the common public interest. Elkin suggests three institutions that could make citizens better deliberators upon the common good: "(1) neighborhood assemblies with significant powers, (2) citywide referenda, and (3) city legislatures, also with significant powers" (p. 171; his focus is on reforms possible at the city level). A politics grounded in these institutions would teach citizens to think of politics as "broadly deliberative" and of political choice as "involving the giving of reasons," not just the aggregation of interests (150, 149). Such an orientation offers the hope of detaching deliberation about the common interest from concern about businesses' bottom line and would encourage citizens to participate constructively in the ongoing task of evaluating, debating, and challenging the policy choices made by city officials so as best to further the common good.

Elkin cites Barber (as well as Rousseau) as the source for his institutional innovations. But the motives he relies on to encourage virtuous public deliberation are quite different. Individuals, he argues, can be brought to act in appropriate, citizenly ways only through the "harnessing of powerful motives": "First, struggle and debate over the public interest must be connected to the day-to-day vital interests of the citizen. . . . Political argument about the public interest must be tied then to specific policy choices . . . that involve such things as neighborhood matters, schools, the land-use patterns of their localities, and a variety of features of their work lives" (1987, 153). This passage captures one of the most admirable features of Elkin's politics of civic virtue: citizens are to be brought to civically virtuous activity through reflection on matters of particular interest to them. The point is to expand the individual's sense of personal self-interest, not to expend resources to subordinate or subdue it. Elkin also calls for eliciting our participation as citizens through engaging the "deep interest that each of us has in enjoying the esteem of others" (ibid.).[12] When civic virtue emerges from motives of this sort, it is privately oriented, because it involves less an engagement with the public than a reflection upon the self.

Three features of Elkin's account of citizenship deserve particular emphasis: (1) he focuses on the deliberative abilities of the citizen, intending to nurture individuals capable of reflecting on the public interest; (2) he tries to nurture the deliberative virtues not by stripping men and women of a sense of private interest but by encouraging them to develop a complementary sense of themselves as citizens, who take pride in properly fulfilling the rights and responsibilities of such a public role; and (3) in sketching out how the institutions necessary to cultivate this sort of civic virtue would work, Elkin works with, rather than abstracts from, self-interest. Neighborhood assemblies, city government, and a city referenda process will focus on matters of day-to-day interest for city residents while invoking their concern with winning the esteem of others. Elkin himself is quite pessimistic about the chances of reviving a civic virtue of the sort he describes (1987, 188); but the possibility of instituting his set of suggested reforms seems far less remote than that of most politics of virtue on offer today.

### Virtue in a "Dualist Democracy"

Bruce Ackerman is another contemporary theorist articulating a privately oriented conception of civic virtue. His account, in the Storrs Lectures, of a "dualist democracy" envisions an American polity in which an "intermittent and irregular politics of public virtue" pursued during "rare periods of heightened political consciousness" alternates with normal politics, in which interests are brokered and factions predominate (1984, 1022).[13] Like Elkin, Ackerman finds it important to reach an accommodation between the self-interest that so dominates our lives and character and the public regard necessary if a liberal democracy is to flourish. Like Cato, Ackerman grounds this accommodation in a two-track vision of citizenship that recognizes "the ebb and flow of political involvement" for the typical "private citizen" (p. 1035). Cato saw the civic virtue of English citizens surfacing only occasionally, drawn forth in frustration and anger when gross corruption and malfeasance threatened their liberty. Between these times of crisis, politics was a matter appropriately left to parliament and the king's ministers. Ackerman envisions a similarly irregular practice of individuals' civic virtue. Most of the time, Ackerman argues, people can and do leave the business of politics to professional politicians willing to engage in the bartering of interests characteristic of normal liberal politics. There will be times, however, when the "ordinary irresponsibilities of politics sometimes begin to offend in a special way" (p. 1040), when normal politics gives way to what he calls constitutional politics. These sporadic efforts to transform the constitutional order, to invoke the higher lawmaking powers of We the People represent, for Ackerman, an American "politics of public virtue"—historically significant moments "when the mass of American citizens mobilizes itself in a collective effort to renew and redefine the public good" (pp. 1023, 1040).

One might argue that on the basis of this description, Ackerman joins Barber and others in advocating a publicly oriented civic virtue. Ackerman himself lays claim to this label, arguing that those who participate in constitutional politics must be understood "to sacrifice their private interests to pursue the common good" (1984, 1020). But the "sacrifice" Ackerman refers to here is simply that of the allocation of time between roles. To expend one's energies on political campaigns to repeal Prohibition, establish the civil rights of America's minorities, or protect unborn children means less time available for everyday obligations and politics, means a sacrifice of private interests. This compartmentalization of personal commitments—what Ackerman calls "the strategy of differential sacrifice" (p. 1041)—is not the sort of sacrifice or subordination of private interest traditionally called for by republican authors. In fact, Ackerman acknowledges that those embarked on the higher lawmaking track will not have "hearts entirely purged of self-interest and minds fully focused upon the 'rights of citizens and the permanent interests of the community' "—such a public orientation of desire and "pristine purity of motive" being, he admits, psychologically impossible (ibid.).

The features that make Elkin's account an important candidate for a contemporary politics of virtue are present in Ackerman's, too. Like Elkin, Ackerman emphasizes the deliberative responsibilities of the good citizen without falling victim to democratic romanticism: the virtuous citizen's participation is called forth only in exceptional circumstances. Ackerman also avoids demanding the sort of disinterested embrace of the public weal that dooms most politics of public virtue. As with Elkin, the good citizen need not jettison his selfish attachments, only place them properly in perspective. The key is to create the circumstances under which individuals come to define themselves as citizens while developing some understanding of the rights and responsibilities entailed in that role. Thus, Ackerman argues, our culture generally provides for, and encourages, our "self-identification as private citizens, capable of responding in a distinctive voice on questions involving the political community," a sensibility whose cultivation becomes a matter of self-interest for the typical American who does not like to "appear to [his or her] fellows as a selfish person bent solely on egoistic self-aggrandizement" (1984, 1033). Thus, our self-interest leads us to virtuous, citizenly behavior.

Ackerman probably assumes too readily that our self-identification as "private citizen" comes naturally in a democratic polity like the United States. A politics of civic virtue conceived along these lines would have to pay a great deal of attention to cultivating a sense of citizenship among citizens. But again, this end is far more easily and less coercively achieved than any effort to develop a truly publicly oriented civic virtue.

Ackerman and Elkin thus offer complementary visions of what it might mean to be a virtuous citizen in the twentieth-century American polity. Their work invites us to think creatively about the enabling conditions of a modern politics of civic virtue—one that is both attractive and achievable, sensitive to the political realities of a mass democracy and alert to the deficiencies of human character.

## CONCLUSION

Some may find it hard to abandon the seductive vision of public virtue—of citizens who manage to make the public (of which whole they are a part) the center of their reflections and affections. Yet imagine what would be necessary to make such a vision a practical reality. The programs by which a truly publicly oriented civic virtue of either the liberal or republican variety might be nurtured in any citizenry are just too demanding, institutionally and psychologically, to gain the lasting assent of the inhabitants of a democratic polity. Those who find this language too melodramatic are urged to consider the circumstances under which the institutions and laws undergirding Rousseau's or Montesquieu's virtuous republics might become law in the United States. With proposals for compulsory national service still too politically controversial to pursue seriously in Congress and with public schools (an important locus of civic education and training) desperately underfunded, where might we expect to find the political will or interest for any comprehensive politics of public virtue? To put this another way, a public virtue available to a democratic citizenry is not the victim of liberalism or commerce, snagged and left behind at some watershed of modernity. The difficulty with the politics of *public* virtue in any democratic regime is simply political: no one will vote for it.

However, those willing to bracket the pursuit of public virtue (for a better and different world) will discover that another sort of civic virtue remains accessible to the modern citizen, one that links our enduring concern for self to the public life of a deliberative citizen. There are a number of ways in which a civic virtue of this sort might be encouraged or grounded, some of which I have explored. The challenge for any modern politics of virtue is therefore to do more than simply shepherd its vision of the good citizen through the perilous byways of democratic debate: it must transform that debate from an impossible emphasis on public virtue to a focus on developing the sorts of privately oriented civic virtue that can and should ground responsible political deliberation and action in the modern polity.

## Notes

1. Budziszewski (1986, 1988) and Will (1980) are two authors that explicitly advocate an Aristotelian politics of virtue. Alasdair MacIntyre (1981, 1990) and Salkever (1990) explore the same territory somewhat less directly.

2. Much of the criticism I shall make applies equally to at least some versions of an Aristotelian politics of virtue. Any criticism of the neo-Aristotelians, however, would have to begin with their conception of the ends of politics, a project I do not undertake here. For my comment on the historical fate of Aristotelian politics of virtue, see Burtt 1990.

3. For the various meanings of public and private in political argument, see Pitkin 1981, 327–52. I use *public* here in a strongly political sense, to refer to the goods and concerns of the *res publica* as considered separately from both the personal and the broadly social.

4. I discuss these separate psychological groundings of civic virtue in Burtt 1990.

5. The phrase "perfect privatist" is Bruce Ackerman's, used with somewhat different connotations in Ackerman 1984, 1033.

6. For a similar argument, see Spitz 1986, 193, 198.

7. On founding, see, esp., Machiavelli's *Discourses* 1.1–10 and Rousseau's *Social Contract* 1.7–10. On corruption, see, esp., Machiavelli's *Discourses* and Rousseau's *Letter to D'Alembert* and *On the Government of Poland*.

8. Barber argues that participation in his program of "civic renewal" can be elicited through "persuasion, through the self-education yielded by democratic participation itself, and through the logic of political priority, which demonstrates that even in a privatistic politics dominated by economic interests, it is only the autonomy of politics and the rights of citizens that give modern women and men the real power to shape their common lives. The taste for participation is whetted by participation: democracy breeds democracy" (p. 265). This final statement may well be true, but does not reach the problem of getting people to participate in the first place.

9. The exception that proves this rule may be aristocratic (i.e., noninclusive) polities where, with the business of everyday life taken care of by slaves, serfs, and women, a select number of men were able to devote themselves either to the practice of politics or to contemplative life. The classic articulation of this possibility is, of course, Aristotle's *Politics*.

10. The account that follows draws on my argument in Burtt 1992, chap. 4.

11. Popular authoritarian regimes of the twentieth century might seem to provide a counterexample to this claim. But Cato's virtuous citizens do not exist in a vacuum. They, too, require a politics of civic virtue, whose conditions are discussed in the next subsection.

12. See Rousseau 1972, Chap. 13.

13. Ackerman 1991 restates and refines this argument.

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# MEDIATED CORRUPTION: THE CASE OF THE KEATING FIVE

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*The Keating Five case exemplifies a form of a political corruption that is increasingly common in contemporary politics but frequently neglected in contemporary political science. I focus on this form by developing a concept of* mediated corruption, *which links the acts of individual officials to effects on the democratic process. Unlike conventional corruption, mediated corruption does not require that the public official act with a corrupt motive or that either public officials or citizens receive improper benefits. The concept of mediated corruption provides not only a more coherent account of the case of the Keating Five but also a more fruitful way of reuniting the concepts of systematic corruption in traditional political theory with the concepts of individual corruption in contemporary social science.*

The case of the "Keating Five"—featuring five prominent U.S. Senators and Charles Keating, Jr., a savings-and-loan financier who contributed to their campaigns—has "come to symbolize public distrust of elected officials" and has reinforced the widespread view that many members of Congress and the institution itself are corrupt.[1] The nine months of investigation and seven weeks of hearings conducted by the Senate Ethics Committee that concluded in January 1992 revealed an underside of our system of representation to a depth and in a detail rarely seen before.

The broad shape of this underside is familiar enough: politicians take money from contributors to get elected, then do favors for them. But the deeper significance, theoretical and practical, is to be found in the details and in the relation of those details to principles of democratic representation. Although the case reveals a darker side of our politics, we can still try to recognize degrees of darkness. We should aim for a kind of moral chiaroscuro. More generally, the case can help us better understand a form of political corruption that is becoming increasingly common but has not received the attention it deserves from political scientists or political theorists.

This form of corruption involves the use of public office for private purposes in a manner that subverts the democratic process. It may be called *mediated* corruption because the corrupt acts are mediated by the political process. The public official's contribution to the corruption is filtered through various practices that are otherwise legitimate and may even be duties of office. As a result, both the official and citizens are less likely to recognize that the official has done anything wrong or that any serious harm has been done.

Mediated corruption is still a form of corruption. It includes the three main elements of the general concept of corruption: a public official gains, a private citizen receives a benefit, and the connection between the gain and the benefit is improper.[2] But mediated corruption differs from conventional corruption with respect to each of these three elements: (1) the gain that the politician receives is political, not personal and is not illegitimate in itself, as in conventional corruption; (2) *how* the public official provides the benefit is improper, not necessarily the benefit itself, or the fact that the particular citizen receives the benefit; (3) the connection between the gain and the benefit is improper because it damages the democratic process, not because the public official provides the benefit with a corrupt motive. In each of these elements, the concept of mediated corruption links the acts of individual officials to qualities of the democratic process. In this way, the concept provides a partial synthesis of conventional corruption (familiar in contemporary political science) and systematic corruption (found in traditional political theory).

To show the value of the concept of mediated corruption, I criticize two interpretations of the Keating Five case that assume a conventional concept of corruption. A concept of mediated corruption, I suggest, provides a better characterization of this case and, by implication, of the many similar cases that have occurred and are likely to occur. The characterization is intended to identify more coherently the aspects of actions and practices that we regard, or should regard, as wrong. The concept of mediated corruption helps bring our considered judgments about corruption in particular cases into "reflective equilibrium" with our moral and political principles.[3]

## WHAT THE KEATING FIVE GAVE AND WHAT THEY GOT

A brief summary of the events in this case will set the stage for examining the competing interpretations.[4] The senators who are forever joined together by the name the Keating Five had never worked together as a group before, and will (it is safe to assume) never work together again. Four are Democrats—Dennis DeConcini (Arizona), Alan Cranston (California), John Glenn (Ohio), and Donald Riegle (Michigan)—and one is a Republican, John McCain (Arizona).

They were brought together by Charles Keating, Jr., now in prison in California, convicted on charges of fraud and racketeering. As chairman of a home construction company in Phoenix, he bought Lincoln Savings and Loan in California in 1984 and began to shift its assets from home loans to high-risk projects, violating a wide variety of state and federal regulations in the process. In 1989, Lincoln collapsed, wiping out the savings of twenty-three thousand (mostly elderly) uninsured customers and costing taxpayers over two billion dollars. It was the biggest failure in what came to be the most costly financial scandal in American history. Lincoln came to symbolize the savings-and-loan crisis.

But to many in the financial community during the years before the collapse, Keating was a model of the financial entrepreneur that the Republican administration wished to encourage through its policy of deregulation. His most visible political lobbying was directed against the new rule prohibiting direct investment by savings-and-loans, which many legitimate financial institutions and many members of Congress also opposed. His most prominent and persistent target was Edwin Gray, the head of the three-member bank board that regulated the industry, himself a controversial figure.

The fateful meeting that would forever link the Keating Five took place on April 2, 1987, in the early evening in DeConcini's office. The senators asked Gray why the investigation of Lincoln and their "friend" Keating was taking so long. Gray said later that he was intimidated by this "show of force." Toward the end of the meeting, he suggested that the senators talk directly to the San Francisco examiners who were handling the Lincoln case. And so they did, a week later, in what was to become the most scrutinized meeting in the hearings. The senators told the examiners that they believed that the government was harrassing a constituent. After the regulators reported that they were about to make a "criminal referral" against Lincoln, the senators seemed to back off.

After that meeting, McCain, Riegle, and Glenn had no further dealings of significance with Keating. Glenn arranged a lunch for Keating and House Speaker Jim Wright the following January, but the committee concluded that although this showed "poor judgment," Glenn's actions were not "improper" (U.S. Senate 1991b, 18). McCain had already broken off relations with Keating, who had called him a "wimp" for refusing to put pressure on the bank board. Cranston and DeConcini continued to act on Keating's behalf.

The Keating Five, particularly DeConcini and Cranston, certainly provided this constituent with good service. Since an act of corruption typically involves an exchange of some kind, we have to ask, What did the Senators get in return? The answer is $1.3 million, all within legal limits.[5] But this figure and this fact, handy for headline writers, obscures some important details (especially the timing and uses of the funds) that should affect our assessment of corruption.

In February 1991, the Ethics Committee rebuked four of the Senators—DeConcini and Riegle more severely, McCain and Glenn less so—and said that further action was warranted only against Cranston. Then in November, after much behind-the-scenes political negotiation, the committee reported to the full Senate that Cranston had "violated established norms of behavior in the Senate." To avoid a stronger resolution by the committee (which would have required a Senate vote), Cranston formally accepted the reprimand. In a dramatic speech on the floor, he also claimed that he had done nothing worse than had most of his colleagues in the Senate.

## COMPETITION OR CORRUPTION?

Cranston's own defense exemplifies, in a cynical form, one of the two standard interpretations of the conduct of the Keating Five. This interpretation holds that the conduct was part of a normal competitive process, in which all politicians are encouraged by the political system to solicit support and bestow favors in order to win elections. We may call this the *competitive politics* theory.[6] On this view, most politicians are not corrupt, nor is the system—even if some citizens like Keating happen to have corrupt designs. The quest for campaign contributions and the provision of service to influential contributors are necessary features of a healthy competitive politics.

The second interpretation also holds that what the Keating Five did is not significantly different from what other members have done but concludes that it is corrupt. On this view (call it the *pervasive corruption* theory), most politicians are corrupt or (more sympathetically) are forced by the system to act in corrupt ways even if they begin with honest intentions.[7] This interpretation is naturally more popular among the press, the public, and academics than it is among politicians. It is consistent with the views both of those who urge radical reforms in the political system (e.g., abolishing campaign contributions completely) and of those who believe that corruption is unavoidable in government (and either accept it or advocate reducing the scope of government).

These two common interpretations seem to be different. Indeed, they seem to be opposites, since one finds corruption where the other does not. But on closer inspection, their concepts of corruption turn out to be fundamentally similar. We can begin to see the similarity in the fact that they both conclude that the conduct of the Keating Five is not morally distinguishable from that of most other politicians.[8] On both accounts, the Keating Five were simply intervening with administrators on behalf of a campaign contributor, a common practice. The competitive politics theory accepts the practice, the pervasive corruption theory condemns it. But on neither theory do the details of the case (e.g., what kind of intervention) make any difference in the moral assessment.

The reason that both theories take this view is that they agree in their fundamental assumptions. The analysis that follows focuses on three of these assumptions (each corresponding to an element in the general concept of corruption) and argues that each is mistaken. Understanding why they are mistaken points toward the need for a concept of mediated corruption.

First, both interpretations assume that corruption requires that the public official receive a personal gain, either directly or indirectly in the form of an advantage that is not distinguished from personal gain. They disagree about whether a campaign contribution should count as personal gain in the required sense, but they agree that some such gain or its moral equivalent is necessary. The image of the self-serving politician acting on base motives contrary to the public interest supplies much of the force of the moralistic reactions to corruption, both the defensiveness of the competitive politics view, and the censoriousness of the pervasive corruption view. This is also the image that most public officials themselves evidently have of the corrupt official: the more personal and the larger the payoff and the less the favor seems part of the job, the more likely is the conduct to be regarded as corrupt (Peters and Welch 1978, 980–81). Second, both interpretations assume that corruption requires that the citizen receive a benefit that is not deserved or be threatened with not receiving one that is deserved. More generally, the justice of the constituent's claim is the only aspect of the benefit that is relevant to the determination of corruption. Third, both interpretations assume that corruption requires a corrupt motive. The personal or political gain and the citizen's benefit are *connected* in the mind of the public official. The official knowingly acts for the contributor in exchange for gain to himself or herself.

## PERSONAL GAIN: THE AMBIGUITY OF SELF-INTEREST

Is personal gain by an official a necessary element of corruption? Only one of the Keating Five—McCain—ever received anything from Keating for his own personal use, and he (along with Glenn) is generally considered to have been the least guilty of the group. (The McCain family took some vacation trips to Keating's Bahamas home in the early 1980s, for which McCain eventually paid when notified by the company in 1989.) If personal gain is an element of the corruption in the Keating Five case, it must be found in the campaign contributions. Should campaign contributions count as the personal gain that the conventional concept of corruption requires?

This case suggests a negative answer. Cranston received no personal financial benefit, yet his conduct was reasonably regarded as the most flagrant of the Five; he was the only one ultimately reprimanded by the Senate. Most of the $850,000 Keating gave to Cranston went to voter registration groups, which had public-spirited names such as Center for the Participation in Democracy and had the purpose of trying to increase turnout in several different states. One of Cranston's main defenses was that he did not gain personally from these contributions.

But one might say that he did gain politically, or at least he thought he would.[9] Why not count this political advantage as the element of personal gain? This is a tempting move and is commonly made, but it should be avoided. It is a mistake to try to force contributions into the category of personal gain. Doing so obscures a moral difference between personal and political gain. These should be distinguished even if one insists on treating both as forms of self-interest. "Personal gain" refers to goods that are usable generally in pursuit of one's own interest (including that of one's family) and are not necessary by-products of political activity. "Political gain" (which may also be a kind of self-interest) involves goods that are usable primarily in the political process, and are necessary by-products of this process.

The distinction is important because in our political system (and any democracy based on elections) the pursuit of political profit is a necessary element in the structure of incentives in a way that the pursuit of personal profit is not.[10] Our system depends on politicians' seeking political advantage: we count on their wanting to be elected or reelected. Among the advantages that they must seek are campaign contributions. If political gain were part of what makes a contribution corrupt, it would also discredit many other kinds of political support, such as organizational efforts on behalf of a candidate, on which a robust democratic politics depends. This is part of the truth in the competitive politics theory.

Some political scientists would offer a more sophisticated rationale for regarding contributions as just another form of personal gain. They would begin with the methodological assumption that politicians act only on self-interest, seeking to maximize their chances of reelection or in other ways to advance their careers. They could then argue that contributions, to the extent that they help achieve these goals, constitute personal gain no less than other goods that further the self-interest of politicians. The trouble with this expansive concept of personal gain is that it does not help identify which contributions should be permitted and which should not. As far as the personal gain is concerned, all contributions are created equal: they are either all proper or all corrupt. If self-interest is viewed favorably (as in some competitive politics theories), the expansive concept would not require that any contributions be prohibited, even those involving what would normally be considered bribery or extortion. If self-interest is viewed unfavorably (as in some pervasive corruption theories), the concept would imply that no contribution should be permitted, even those serving what would normally be regarded as the public interest. In either case, the self-interest assumption does not itself supply any way to distinguish legitimate from illegitimate pursuit of personal gain.

Neither could we get much help with this difficulty from a more refined model based on principal–agent theory, which is sometimes used to analyze corruption.[11] On this model, the politician acts as an agent for constituents, the principals. Because of the costs of monitoring and other factors, the principals cannot reliably control the agent's actions. In the absence of other constraints, the resulting "slack" allows the agent to act on his or her own interest contrary to that of the principals. The model could help us see that corruption may be partly the result of the structure of incentives in the system: agent–principal slack creates moral hazards that permit corruption. But the model is neutral between proper and improper behavior. It is applicable to corrupt principals and agents as to honest ones. It could treat the Keating Five as agents of Keating in carrying out corrupt purposes. It does not explain why we should have a system that allows some kinds of incentives (contributions) and not other kinds (bribes). It might be said, of course, that taking bribes has more socially harmful consequences than accepting campaign contributions. But if this is the claim, then what is wrong is no longer the personal gain, but a certain *kind* of personal gain; and it is wrong not because it is a *personal* gain at all but because of its effects on the system.

If the presence of personal gain is not necessary to make a contribution corrupt, neither is its absence sufficient to make a contribution correct. Consider this hypothetical example. Suppose that after meeting with Mother Theresa (which in fact he did), Keating decides in a fit of saintly fervor that a portion of his campaign contributions and those of others he solicits for the Keating Five should go secretly to a government trust fund to support new programs to help the poor. Suppose, further, that the Keating Five, respecting this act of charity by a constituent, work together behind the scenes to establish the Mother Theresa Fund for this purpose. (To add a further touch of irony, let the fund be administered, at Keating's request, by Ed Gray.) None of the senators would have gained personally, and a good cause would have been served.[12] Would there be any grounds for concern about corruption?

There surely would be some. Keating would have managed to promote a private project with the aid of public officials but without the warrant of the democratic process. The cause, however noble, was not one that citizens or their representatives had chosen through legitimate procedures. Acting on principle for higher causes can be no less corrupting—and may be even more dangerous—than acting for personal gain because the perpetrators are more likely to be able to enlist others in their schemes. Oliver North would have not been able to mobilize so much support for his projects in the Iran–Contra affair had he been acting mainly for personal gain.

## OFFICIAL FAVORS: THE PERILS OF CONSTITUENT SERVICE

Consider now the second element of corruption, the official favors that the senators provided. The Keating Five claimed that there was nothing improper about the help they gave Keating. The benefits that Keating received were all provided in the name of "constituent service," a normal practice in a political system where representatives have to compete for the support of voters and campaign contributors.

The senators—and even the Ethics Committee at times—seemed to assume that if what a member does is constituent service and breaks no law, it is never improper. If the conduct does not involve bribery, extortion, or an illegal campaign contribution, it is not only acceptable but admirable.[13] This is the competitive politics theory in its purest form. But as the hearings progressed, some of the senators came to accept a slightly more moderate view. In effect, they allowed that otherwise proper constituent service could become improper if it were provided unfairly. It would be wrong (and perhaps evidence of corruption) if it were provided only to big contributors.[14] The senators seemed to accept as a reasonable test the question, Does the member typically intervene in this way for other constituents?

DeConcini made it a major part of his defense to show that he responded to virtually any constituent who asked for help. (He brandished a list of 75,000 constituents who could be called to testify, though to everyone's relief he settled for inviting only three—a social worker for Hispanics, a drug-busting sheriff, and a handicapped veteran.)[15] Despite these heroic efforts, the answer to the question in this case is still probably negative: what the Keating Five provided was not typical constituent service. Five senators meeting in private with regulators on a specific case is unusual. During the hearings, no one could cite a sufficiently close precedent.[16]

But even if we were to accept that the senators would do for other constituents what they did for Keating, we should still be concerned about another feature of this case, what may be called "the problem of too many representatives." Only DeConcini and McCain could claim Keating as a constituent in the conventional (electoral) sense. The other three count as his representatives mainly by virtue of his business interests in their states.[17] It is true that business interests, like other interests, may deserve representation; and geographical districts need not define the limits of representation, even of constituency service. However, we may reasonably criticize multiple representation if, in practice (as this case suggests), the extra representatives tend to go disproportionately to those with greater financial resources. That this tendency is undesirable is part of the truth in the pervasive corruption theory. A fair system of democratic representation does not grant more representatives to some citizens just because they have more financial resources.

So far, these criticisms could be consistent with the concept of conventional corruption, which assesses the benefit only in relation to the justice of the constituent's claims. Mediated corruption goes further and considers the effects on the policymaking process—most importantly, the foreseeable reactions of other officials (in this case, the regulators, staff, and other members). Instead of asking whether a member would provide this benefit equally for any constituent, mediated corruption asks whether the benefit should be provided in this way at all.

Unmentioned in the Constitution, unimagined by the founders, and until recently unanalyzed by journalists, constituent service has become a major part of the job of most members of Congress (Cain, Ferejohn, and Fiorina 1987, 50–76; and Fenno 1978, 101). It serves some valuable functions, the most important of which, perhaps, is to provide a check on abuse of power by executive agencies in individual cases—in effect, fulfilling the role played by an ombudsman in some other political systems.[18] If administrators are "harrassing a constituent," as some of the senators said they suspected in this case, members may be obligated to intervene not only to protect the constituent but also to correct administrative procedures.

Yet, as recent scholarship has emphasized, constituent service is not a wholly beneficial practice even when legitimately performed (Cain, Ferejohn, and Fiorina 1987, 197–229). Even if the casework done by each individual member is perfectly proper, the collective consequences may not be so beneficial for the system as a whole. One danger is that as constituent service becomes such a prominent part of the job, legislative duties suffer. Voters tend to pay more attention to personalized service than to legislative records, and political responsibility for these records withers. Another danger is that by concentrating on righting wrongs against individual citizens, constituent service can favor particular remedies over general reforms. Ad hoc and local solutions do not necessarily produce changes in procedures or policies that benefit the public as a whole. Yet another danger is that to the extent that incumbents gain electoral advantage through constituent service, new members who might bring fresh policy views or offer new criticisms of government performance are less likely to make their way into the legislature.

Once we accept that constituent service, quite independently of campaign contributions, is a mixed democratic blessing, we can see that to justify any act of constituent service, it is not sufficient to point to the benefit to particular constituents or even to the value of the practice for the system in general. Standards to assess constituent service are best derived from principles of legislative ethics, which identify the general characteristics that a system of representation should have in order to provide conditions for making morally justifiable decisions (Thompson 1987, chap. 4). Three principles—generality, autonomy, and publicity—yield three sets of standards.

Standards of *generality* require that legislative actions be justifiable in terms that apply to all citizens equally. These standards refer most directly to legislation itself, where they favor actions that provide public goods for a broad class of citizens over those that confer private advantage on individual citizens. They also have institutional implications, one of the most important of which is the separation of powers, a chief purpose of which is to maintain the appropriate level of generality by assigning to branches other than the legislative the role of applying the laws. Legislative actions that are appropriate in the process of making laws may not always be appropriate in the process of administering them. Some of the most important standards of legislative ethics, therefore, in various ways prescribe that any legislative intervention should be appropriate to the administrative proceeding in question.

In the Keating case, the senators evidently did not recognize any difference between what would be an appropriate intervention in rule-making proceedings and what would be appropriate in quasi-adjudicatory proceedings (which the Lincoln case resembled). In the latter kind of proceeding, there is generally more procedural protection for constituents and less legitimate scope for disputes about policy.[19] Some political bargaining, of course, is necessary in the administrative process; but at least some of the senators in this case went beyond what we might call the "normal range" of acceptable political pressure. They did more than make status inquiries (which are perfectly proper). To the regulators, their conduct looked more like a threat (specifically, a threat to oppose a pending bill to fund the savings-and-loan bailouts, which was generally believed to be urgently needed).

Standards of *autonomy* prescribe that representatives act on relevant reasons (Thompson 1987, 111–14). They require that any intervention be appropriate to the substantive merits of the constituent's case. Such standards would not prohibit members from acting aggressively on behalf of constituents, but they would clearly direct members to consider the substance of constituents' claims in deciding whether and how to intervene. Members may have a duty to support meritorious claims; but they should not press claims that they have, or should have, reason to believe are without merit. Furthermore, the higher the stakes, the greater the responsibility to investigate the merits of a claim. More generally, simply pressing claims without any regard to their merits is to promote a policymaking process moved more by considerations of power than of purpose.[20]

Many members may not recognize an ethical problem in intervention, because they take their roles to be like a lawyer's, whose duty is assumed to be to press a client's case without regard to merits. They may consider the only alternative to be to play a judge's role, which would allow no scope for partiality toward their constituents. But the role of the representative differs from both, permitting members to give special consideration to their own constituents provided that they take into account the effect on other citizens and on the public generally.

Keating's case, it is now known (and could have been known then) lacked merit. Glenn and McCain made some effort to find out about the case before they went to the meetings with Gray and the regulators. Giving the senators the benefit of the doubt, we might say that under the pressures of time they took all the steps one could reasonably expect before they intervened with the regulators. The senators did, after all, have some evidence at the time (a letter from Alan Greenspan and a statement from the firm of Arthur Young) that appeared to lend credibility to Keating's complaints. But once the senators heard about the regulators' intention to make a criminal referral, they had adequate notice that his claims were questionable. Although criminal referrals are not unusual and are not clear evidence of wrongdoing, a conscientious senator (indeed, even a prudent one) would have looked more closely into the merits of Keating's case before continuing to assist him. The only two who continued to press his case, DeConcini and Cranston, did not. This is partly why their conduct could be criticized more severely, as the special counsel (and, less clearly, the Ethics Committee) suggested.

Standards of *publicity* require that an intervention take place in ways that could be justified publicly. It is, of course, neither practical nor desirable that all interventions be formally on the record and made public at the time. But the intervention should not be so clandestine that the member and the agency cannot be held accountable should the action later be called into question.[21] This is why some members put in writing their inquiries to administrative agencies and keep a record of other similar contacts.

The interventions for Keating, though not strictly secret, fell short of meeting the publicity principle. The pattern of the interventions—after-hours meetings, the absence of aides, early morning phone calls to regulators at home, the vagueness of records (except for the famous "transcript" of the April 9 meeting)—made it difficult to reconstruct the events. They create a reasonable suspicion that the discussions were never intended to be acessible for public review.

The constituent service that the senators provided Keating, as measured by these standards, did not serve the democratic process well. To this extent—and in varying degrees in the case of each senator—the benefits that Keating received qualify as improperly provided; they count as the second element of mediated corruption. With the concept of mediated corruption, then, we can criticize some forms of constituent service for contributors without rejecting all forms as does the pervasive theory, or accepting all forms as does competitive theory.

## CORRUPT CONNECTIONS: THE SIGNIFICANCE OF MIXED MOTIVES

In any form of corruption, there must be an improper connection between the benefit granted and the gain received. Otherwise, there would be only simple bias or simple malfeasance. With conventional corruption, we look for the link in the guilty mind of the public official—a corrupt motive. But the question immediately arises, How can we distinguish corrupt motives from other kinds? We have already seen that personal gain is neither a necessary nor a sufficient condition, nor is the impropriety of the benefit. Therefore, the corruption has to be found partly in the nature of the exchange. The difficulty is that corrupt exchanges do not seem obviously different from many of the other kinds of deals that go on in politics—exchanges of support of various kinds, without which political life could not go on at all. Politics is replete with quid pro quos: you vote for my bill, and I'll support yours; you raise funds for my primary campaign, and I'll endorse you in the next election. What is so corrupt about the exchange of campaign contributions for constituent service? Without an answer to this question, we would be forced either to brand nearly all ordinary politics as corrupt or to excuse much political corruption as just ordinary politics.

The competitive politics theory, fearing a purification of the process that might enervate political life, attempts to contain the concept of corruption. It insists on narrow criteria for what counts as a corrupt link. The connection between the contribution and the service must be close in two senses: proximate in time and explicit in word or deed. In the case of several of the Keating Five, the connection between the contributions and the service were, by these standards, close. The connection was especially close in the case of Cranston, one reason that the committee singled him out for special criticism. He solicited contributions from Keating while he was also working to help Lincoln with its problems. His chief fundraiser combined discussions about regulations and contributions. Favors and contributions were also linked in memos and informal comments. He made the connection explicit in a memorable line delivered at a dinner at the Belair Hotel, where he "came up and patted Mr. Keating on the back and said, 'Ah, the mutual aid society'."[22]

The committee found the contributions and services to be "substantially linked" through an "impermissible pattern of conduct," but they stopped short of finding "corrupt intent" (U.S. Senate 1991b, 36). Why did the committee decline to find corruption here? The connection, it would seem, could hardly be closer. For that matter, we might also ask why the committee did not find the pattern impermissible in the case of Riegle and DeConcini. Part of the answer probably is that "corrupt intent" is the language of the bribery statutes, and the committee did not dare suggest that campaign contributions could be bribes. The line between contributions and bribes must be kept bright.

But *is* the line so bright? "Almost a hair's line difference" separates bribes and contributions, Russell Long once testified (Noonan 1984, 801). Courts have not been able to provide a principled way of distinguishing the two.[23] There is, furthermore, no good reason to believe that connections between contributions and benefits that are proximate and explicit are any more corrupt than connections that are indirect and implicit. The former may be only the more detectable—not necessarily the more deliberate or damaging—form of corruption.

Are we driven, then, to accept the conclusion of the pervasive corruption theory on this point, that virtually all contributions are corrupt? This theory is right to insist that corruption should be viewed in the context of the political system, that it can work through patterns of conduct, institutional routines, and informal norms. But the theory does not encourage the kinds of distinctions that seem necessary in the kind of politics we actually have and are likely to have in the foreseeable future. It treats, for example, the explicit, proximate bribe the same as the routine thousand-dollar campaign contribution to one's longtime party favorite.

Both the competitive politics and pervasive corruption theories assume the criterion of the corrupt connection to be the actual motives of the citizens and politicians. This assumption is mistaken, because it ignores an important structural feature of representative government. Any electorally based representative system permits—indeed, requires—representatives to act on *mixed* motives. They act for the benefit of particular constituents, for the good of the whole district or state, for the good of the nation, and for their own interest in reelection or future political ambitions. Some of these motives may be more admirable than others; but none is illegitimate in itself, and all are necessary in some measure in our system.

Under the circumstances of mixed motives, it is hard enough for any official, however conscientious, to separate proper from improper motives and, more generally, to find the right balance of motives in making any particular decision (see Douglas 1952, 44, 85–92). It is harder still for citizens—even well-informed and nonpartisan ones—to judge at a distance whether the official has really found that balance. Therefore, in the design of a representative system or in the practice of judging representatives, we cannot in general count on being able to evaluate motives in individual cases.

What we need, instead, is a standard that assesses an individual official's action in the context of the system as whole. The standard should identify systematic tendencies that we know from past experience are likely to lead to corruption. It would then refer to the motives on which any official in the circumstances may be presumed to act, instead of the motives on which this official actually acts. It is just this kind of logic that underlies the so-called appearance standard, at least when properly interpreted.

In a plethora of codes, rules, and statutes that regulate the ethics of government, public officials are now enjoined to avoid the appearance of impropriety (U.S. Senate 1991b, *Additional Views*, 14–16). The term *appearance* is unfortunate, however, as it encourages misinterpretations of the standard.[24] The mere appearance of an ethical wrong is contrasted with a real wrong, and the violation of the appearance standard is taken to be a minor, lesser offense, a sort of pale reflection of the real offense. The standard then comes to be regarded as merely prudential, a piece of political advice, which, if not followed, is seen as grounds for a charge of mistaken judgment, rather than an ethical wrong. Notice that the Ethics Committee found that DeConcini and Riegle, whom they charged with creating an appearance of impropriety, showed only "insensitivity and poor judgment" (U.S. Senate 1991b, 17, 19).

Properly interpreted, the appearance standard identifies a distinct wrong, quite independent of—and potentially no less serious than—the wrong of which it is an appearance. It would be better called a "tendency" standard, since it presumes that under certain conditions, the connection between a contribution and a benefit *tends* to be improper. The standard seeks, first, to reduce the occasions on which the connection is improper in the conventional sense. These are the occasions on which the provision of the benefit is actually motivated primarily by the contribution. But the second, more distinctive, aim of the standard is to decrease the occasions on which the connection is reasonably perceived to be improper. This perception is grounded on our general knowledge of the conditions that tend to produce actual improper connections, but the wrong is based on a different kind of moral failure. When an official accepts large contributions from interested individuals under certain conditions, whether or not the official's judgment is actually influenced, citizens are morally justified in believing the official's judgment has been so influenced and acting on that belief themselves. The official is guilty of failing to take into account the reasonable reactions of citizens.[25]

The justification for this kind of standard should be distinguished from the type of argument (common in discussions of rule utilitarianism) that justifies particular acts by appeals to general rules or policies. In the rule utilitarian argument, an overly broad rule is justified by showing that the costs of deciding each case are greater than the costs of wrongly deciding some cases. The argument for the appearance standard differs in two respects. First, it counts as a cost not simply the risk that a case might be wrongly decided but also the likelihood that the public will perceive the case to have been wrongly decided. Public confidence could be undermined, and misconduct by others encouraged, *even if a case were rightly* decided but not so perceived. Second, the rationale for the appearance standard rests in part on a publicity principle, which holds that the reasons on which public officials may be assumed to act should be accessible to citizens. Appearances, then, are in these ways valuable beyond their role as evidence for corrupt motives.

Because appearances are often the only window that citizens have on official conduct, rejecting the appearance standard is tantamount to denying democratic accountability. This was dramatically demonstrated in the objections frequently raised during the hearings by several of the senators and their attorneys. Cranston, most notably, kept objecting to the idea that his conduct should be judged by how it *appears* to a reasonable person. That is a "mythical person," he said. The only *real* person who can judge is the senator himself. "You were not there. *I* was there. And *I* know that what I knew at the time . . . convinced me that my [actions] were appropriate" (U.S. Senate 1991a, pt. 1, November 16, 1990, pp. 121–22).

The appearance standard, however, does not itself identify the kinds of conditions that would warrant a conclusion of mediated corruption but only points in the right direction—away from actual motives to presumed motives and objective intentions and thereby to the tendencies or conditions that create corrupt connections in democratic systems. We still need some basis on which to distinguish corrupt from noncorrupt contributions.

We can begin to see the basis for such a distinction in the common reaction that there was something peculiar about Keating's lavishing support on senators whose political views he so strongly opposed. Cranston and Keating were an odd couple: an archconservative Arizona businessman devoted to the free market and opposed to pornography and abortion teamed up with one of the leading liberals in the Senate, a former candidate for president who had called for a nuclear freeze and higher social spending. The two differed even on government policy toward the financial services industry.

This ideological incongruence is significant not because it exposes cynical or self-interested motives but because it reveals apolitical practices. Specifically, it identifies a type of contribution that serves no public political function. A contribution given without regard to the political positions of the candidate only incidentally provides political support. Its aim is primarily to influence the candidate when in office. In its pure form, it has no function other than to translate the desires of a contributor directly into governmental action. In effect, it short-circuits the democratic process. Contrast this kind of contribution with one given to support a candidate with whom one shares a general political orientation or agrees on issues that one thinks salient.[26] A contribution of this kind directly serves a political function: its aim is to help a candidate get elected, and it works through the political process.[27] Rather than bypassing the process, the contribution animates it.

Neither the pervasive corruption nor the competitive politics view can easily distinguish these types of contributions (though the competitive politics view could recognize an analogous distinction between contributions that further the competitive process and those that do not). The basis of the distinction is the principle that citizens should influence their representatives—and representatives should influence policy—only in ways that can be contested through public discussion in a democratic political process. This principle is consistent with a wide range of conceptions of representation. The problem with the first kind of contribution, the Keating type, is not that it makes a representative an agent of individual constituents but that it makes the representative an apolitical agent. The objection is not only that the contributions come with strings attached but also (even more insidiously) that the strings have no political substance.[28]

Ideological incongruence, which is common enough in current campaign finance (as big contributors hedge their electoral bets), is not itself a necessary or sufficient condition of corruption. Neither does its absence make an otherwise questionable practice acceptable. However, its presence is a strong indication in any particular case that corruption may also be present. More generally, the concept points toward a comprehensive criterion for identifying corrupt connections.

The connection between contributions and benefits is corrupt if it bypasses the democratic process. The corruption here is twofold. It consists, first, in the actual and presumed tendency of certain kinds of contributions to influence the actions of representatives without regard to the substantive merits of issues. This is the corruption of the representative's judgment. The corruption also shows itself in broader effects on the democratic process, namely, in the actual and presumed tendency to undermine substantive political competition and deliberation. This is the corruption of the representative system.

There are, of course, many different ways in which contributions might be regarded as undermining the democratic process; and which ways we build into the concept of corruption will depend on what conceptions of democracy we accept. The principles of legislative ethics invoked earlier to assess forms of constituency service could be used again here to generate some criteria for identifying corrupt connections. We could then hold that a connection is more likely to be corrupt, (1) the more particular the aim of the contributor, (2) the less closely the contribution is connected to the merits of conduct it is intended to influence, and (3) the less accessible the exchange is to publicity. Each of these criteria would have to be specified more fully and translated into enforceable standards before we could conclude that we have a satisfactory test for the corrupt connection in mediated corruption.[29] But it should already be clear that the criteria presuppose an approach that makes the relationship to democratic processes more fundamental than do conventional approaches to corruption.

## CONCLUSION

Mediated corruption is not new, but it is newly prospering. It thrives in the world of large, multinational financial institutions that increasingly interact, in closed and complex ways, with governments. Many of the major governmental scandals in recent years have involved a large measure of mediated corruption—the affairs of Iran–Contra, Housing and Urban Development (under Samuel Pierce), the Bank of Credit and Commerce International, and the Banca Nazionale del Lavoro's Atlanta branch, among others. Where private greed mixes easily with the public good, where the difference between serving citizens and serving supporters blurs, where secret funds lubricate the schemes of public officials, there mediated corruption is likely to flourish.

We can better understand the cunning ways of this growing form of corruption if we keep in mind its distinctive characteristics. The concept of mediated corruption serves this purpose. Each of its three elements, it has been argued here, differs from those of conventional corruption, the kind assumed by the competitive politics and pervasive corruption theories.

First, in mediated corruption a public official typically receives a political gain. But, as the pervasive corruption mistakenly denies and the competitive politics theory rightly implies, there is nothing wrong with this gain itself. Mediated corruption, furthermore, does not require that the public official personally gain or otherwise serve his (narrow) self-interest, as conventional corruption typically assumes. The gain contributes to mediated corruption insofar as it damages the democratic process—for example, by influencing a representative to serve private purposes without regard to their substantive merits.

Second, the public official provides a benefit, typically as an intermediary attempting to influence other officials to serve a constituent's private ends.[30] Contrary to both the competitive politics and the pervasive corruption views, the benefit itself may be deserved and may even be something that the official would provide for any constituent. But if the way in which the official provides the benefit damages the democratic process, it still counts as a contribution to the corruption. The way in which the member presses the constituent's claim, not simply the justice of the claim, is relevant to the assessment of the corruption.

Third, the connection between the gain and the benefit is corrupt if it would lead a reasonable citizen to believe that an exchange has taken place that damages the democratic process in specified ways (typically in ways that bypass the process). Mediated corruption thus adds an appearance standard to the corrupt-motive test of conventional corruption. It goes further, and, like the pervasive corruption theory, relates the corruption in any particular case to corruption of the system as a whole. But the standards for determining whether the connection is corrupt are more fine-grained than that theory allows. They permit some connections that might otherwise seem corrupt (e.g., money is not necessarily corrupting) and condemn some connections that otherwise seem legitimate (money can be corrupting independently of the inequalities it perpetuates). The standards ultimately depend on what kind of democratic process we wish to maintain.

The concept of mediated corruption is consistent with a wide range of theories of democracy but is probably best justified from the perspective of a theory that prescribes that officials act on considerations of moral principle, rather than only on calculations of political power. This is sometimes called the deliberative conception of democracy (see Cohen 1989, 17–34; Larmore 1987, esp. 59–66; Manin 1987).[31] As we have seen, mediated corruption characteristically attempts to translate private interest directly into public policy, bypassing the democratic processes of political discussion and competition. It thereby blocks our considering the moral reasons for and against a policy. Mediated corruption also prevents *deliberately* adopting a policy even without considering moral reasons: it precludes deliberation about whether to deliberate.

If we accept the concept of mediated corruption (as a supplement to the concept of conventional corruption), at least three implications follow. First, cases like the Keating Five would look different in the future. It would be easier to justify making finer distinctions of kind and degree in judging misconduct. The kind of conduct in which McCain and Glenn engaged, for example, would be more clearly distinguishable from that of the other three; and the kind of conduct in which all five engaged would be more clearly set apart from that of most other senators. More generally, we would hear less talk of motives (whether honest or rationalized), fewer appeals to constituent service as if it excused all sins, and fewer attacks on the appearance standard. We would see more concern about the mixing of private profit and public service, more attention to the merits of constituents' claims, and more worry about the effects of practices of individual representatives on the broader process of democratic representation. This shift of attention from the individual to the system (or, more precisely, to the effects of individual behavior on the system) would require not only new ways of thinking but also new standards of ethics.

The practical change most emphasized by the committee and most often mentioned by observers is campaign finance reform. Reducing the importance of money in campaigns (and politics more generally) is certainly desirable and could be seen as one of the implications of the concept of mediated corruption. But since the dominating role of money in politics is objectionable from the perspective of many different theories, it is worth emphasizing an implication that points toward a different dimension of reform. The concept of mediated corruption helps bring out the fact that money is not the only important source of corruption. Some of the kinds of misconduct to which mediated corruption calls attention depend less on money than do the kinds condemned by conventional corruption. As far as *public officials* are concerned, mediated corruption works its wiles less through greed than through ambition and even a misplaced sense of duty. Even some quite radical campaign finance reforms would not completely eliminate some forms of mediated corruption. Recall that political action committees, the bête noire of many progressive reformers, played almost no role in the case of the Keating Five.

A second implication of adopting the concept of mediated corruption concerns the process by which charges of unethical conduct should be heard and decided. In the Keating Five case, the process was directed by the Ethics Committee of the Senate. Legislatures have traditionally insisted on exclusive authority to discipline their own members, and the ethics committees of both houses have in the past managed to bring some tough judgments against some of their colleagues. But these have almost always been in flagrant cases of wrongdoing, closer to clear violations of rules that resembled the criminal law. It is difficult enough for colleagues who have worked together for years and may have to work together again to bring themselves to judge one another harshly in these cases. It may be almost impossible in cases involving mediated corruption. The less the charge is like conventional corruption, the harder it is to reach a severe judgment. The member implicated in mediated corruption, showing no obvious signs of a guilty mind or especially selfish motives, is often seen as simply doing his job. Under such circumstances, the sympathy of one's colleagues is maximized, their capacity for objectivity, minimized.

Furthermore, the legislature is, in a sense, also judging itself—specifically, its own practices and procedures, through which the corruption is mediated. In these circumstances, we might reasonably wonder whether anybody, including a legislative body, should be a judge in its own case. The clear implication of these considerations, suggested in part by the concept of mediated corruption, is that we should consider establishing an outside body to judge cases of ethics violations. To overcome possible constitutional objections, Congress could ultimately control the body; but it should be established in a way that would have at least the independence and respect of an institution like the Congressional Budget Office.

The third implication is methodological in character and is perhaps the most significant for the study of corruption and democracy. The concept of mediated corruption has the potential to integrate the very different approaches to corruption that prevail in political theory and in social science. The difference was strikingly illustrated some years ago when the *American Political Science Review* published a pair of articles on corruption, one by a political theorist and one by two political scientists (Dobel 1978; Peters and Welch 1978). The editor perhaps intended to take a step toward unifying the discipline by putting the articles together in the same section. Yet the articles had little in common except the common word *corruption* in their titles; the authors might as well have been writing about different subjects.

The political theorist faithfully followed his tradition, invoking Machiavelli, Rousseau, Montesequieu, and Madison (among others), and presented corruption as a characteristic of a political system as whole. He saw it as a sickness of the body politic, a turning away from civic virtue toward private interests. It may afflict individual citizens and their rulers, but it can only be fully understood from the perspective of the whole society.[32] The political scientists described corruption in terms of transactions between individuals. The transactions, of course, take place within a system; and the system may be called corrupt when its structures and incentives encourage corruption. But the basic unit of analysis remains an exchange between individual officials and individual citizens.

Mediated corruption holds the promise of putting back together the structuralist and the individualist conceptions of corruption that these intellectual traditions have split apart. The integrating instrument, it has been suggested here, is the idea of the democratic process. With mediated corruption, we cannot decide whether corruption exists, let alone how serious it is, without paying attention to its effects on the democratic process and therefore without making moral judgments about the kind of democratic process we wish to encourage. The concept of mediated corruption permits a conclusion that corruption is pervasive and the system needs radical reform; but the grounds of the conclusion, as well as the nature of the reform, would be guided by a conception of the democratic process. Mediated corruption also supports judgments about individuals competing within the existing system; these, too, are to be shaped by a view of the democratic process. We cannot assess either patterns of systematic corruption or instances of individual corruption without presupposing a theory of democracy.

Because the concept of mediated corruption is theory-dependent in this way, we should not suppose that we can understand corruption without making value judgments about politics. In this respect, those social scientists who try to justify corruption in some societies as necessary to achieve certain political values (such as efficiency or social integration) are right about the structure of the argument required to assess corruption.[33] Whether certain kinds of conduct should count as corrupt depends in part on its net effects on the political system as a whole. However, in making their calculations, these social scientists tend to give too much weight to outcome values relative to process values, a mistake that the approach of mediated corruption avoids.

The social scientists who find corruption functional also tend to assume that their methods are objective and realistic while the methods of those who criticize corruption are subjective and moralistic. But even if we were to accept certain kinds of corruption as functional, we would still be making a moral judgment. As this search for the meaning of corruption in the case of the Keating Five should make clear, on the subject of corruption, we are all moralists. The only question is what kind of moralists we want to be. Unlike the Keating Five, their apologists, and even most of their critics, we should try to be *democratic* moralists.

## Notes

During part of the investigation and hearings in this case, I served as a consultant to the special counsel to the Senate Ethics Committee, Robert S. Bennett. I am grateful to him and

members of his staff, especially Benjamin Klubes, for helpful advice and convenient access to public documents. I have also learned from discussion with Senator Cranston's attorney, William W. Taylor III (though he may believe not enough). I have benefited from valuable comments on various versions of this article from Ted Aaberg, Arthur Applbaum, Charles Beitz, Morris Fiorina, Charles Fried, Amy Gutmann, Sanford Levinson, Thomas Scanlon, Judith Shklar, Alan Wertheimer, David Wilkins, and Kenneth Winston. An earlier version of this article was presented at Princeton University under the auspices of the University Center for Human Values.

1. Berke 1991. Although other factors no doubt contributed to the low public regard for Congress in this period, the percentage of respondents who rated the honesty and ethical standards of senators "very high" or "high" declined by five points between 1990 and 1991; and the rating of Congress itself fell to an all-time low (Hugick 1991; Hugick and Hueber 1991). Other surveys found that a majority of respondents believed that at least half of the members of Congress are "corrupt" and that the institution itself is "corrupt" (CBS News/*New York Times* 1991; NBC News/*Wall Street Journal* 1992). The latter survey found that of the "scandals and controversies that have taken place during the past few years in Washington," the handling of problems with the savings-and-loan industry "most bothered" more respondents than any other.

2. This general concept is meant to be consistent with a wide variety of definitions in the social science literature. However, further specification of the concept beyond this level of generality remains controversial (mostly with regard to what should count as "improper"). (It should be noted that in some forms of conventional corruption, the "public official" and "private citizen" may be the same person.) For a review of various approaches, see Heidenheimer, Johnston, and LeVine 1989, 7–14; Peters and Welch 1978, 974–78.

3. The most influential explanation of reflective equilibrium is Rawls 1971, 48–51. A more recent systematic account is Richardson 1990.

4. No one is likely to mistake this summary for authoritative history. It is intended to serve only as a simplified reminder of some of the highlights of the case. The summary (as well as subsequent comments about the case) relies primarily on the evidence presented during the hearings and in the reports of the special counsel and the Ethics Committee. See U.S. Senate 1991a, 1991b. Two readable accounts of the affair, including some useful background material, are Adams 1990, pt. 4; Pizzo et al. 1989, 263–97.

5. Keating acted often as a broker for others, sometimes as a "bundler," taking "the separate individual contributions and bundling them together, . . . claiming credit for the harvest" (Sorauf 1992, 54). This is yet another way in which the corruption in this case could be regarded as mediated.

6. Examples of this interpretation typical of many public comments on the affair are Yoder 1991 and testimony by Senator Daniel Inouye (U.S. Senate 1991a, pt. 3, December 3, 1990, pp. 2–50). Similar views can also be found in court opinions and in more general discussions of campaign reform (e.g., *McCormick* v. *U.S.* 1991, 1825; Gottlieb 1989). The competitive politics view is also consistent with a number of well-known analyses of corruption (e.g., Banfield 1975; Wilson 1974, 29–38). Although the view might be supported by various democratic theories, the version of pluralist theory (sometimes called the competitive theory of democracy) is its most natural ally. The classic statements are those of Schumpeter (1962) and Downs (1957).

7. Typical examples of this interpretation from public comment on the case are Abramson and Rogers 1991, Etzioni 1990/91, and Wilkinson 1991. Also see the affadavit of Senator Ernest Hollings (U.S. Senate 1991a, Exhibits of Senator DeConcini, 493–95). In the literature on campaign reform and political corruption more generally, the interpretation would find support in several different analyses (e.g., Etzioni 1984; Lowenstein 1985, 826–28; Lowenstein 1989, 301–35; Noonan 1984, 621–51). For discussions of democratic theory that could be used to support this view, see Cohen and Rogers 1983; Dahl 1989, chap.9; Lindblom 1977, pt. 5.

8. This conclusion is evidently widely accepted by the public. Asked in an NBC/*Wall Street Journal* poll whether they thought that the ethical violations of which the Keating Five were accused are "typical" of the behavior of Senators and members of Congress, 71% of the respondents agreed that they were; 19% disagreed.

9. The conventional wisdom that higher turnout helps the Democrats has been challenged. See DeNardo 1980; Tucker, Vedlitz, and DeNardo 1986.

10. Compare Madison's observation in *Federalist* 51: "Ambition must be made to counteract ambition. . . This policy of supplying, by opposite and rival interests, the defect of better motives, might be traced through the whole system of human affairs" (Hamilton, Madison, and Jay 1961, 349).

11. A pioneering work that exemplifies both the strengths and weaknesses of this approach is Rose-Ackerman 1978, 6–10.

12. A variation on this hypothetical example actually took place. According to the testimony of James Grogan, a Keating aide, Senator DeConcini's wife at times solicited contributions from Keating for her favorite charities in the community (U.S. Senate 1991a, pt. 4, December 14, 1990, pp. 248–49).

13. U.S. Senate 1991b, 14–16; idem 1991a, pt. 1, November 16, 1990, pp. 126–32 and November 19, 1990, pp. 91–92.

14. U.S. Senate 1991a, pt. 1, November 16, 1990, p. 111 and November 19, 1990, p. 23.

15. Ibid., pt. 4, December 10, 1990, pp. 58–94.

16. Lobbying the Defense Department to support Apache helicopters, asking the Customs Service for an exception to trade restrictions, questioning the Justice Department about a potential indictment of a shipyard company (U.S. Senate 1991a, pt. 1, November 19, 1990, pp. 14–17; idem, pt. 4, December 10, 1990, pp. 9–12; idem, pt. 6, January 10, 1991, pp. 137–40)—these examples and all of the others paraded before the committee lacked some critical feature of the Keating case. None involved pressure on independent regulators to give special treatment to a particular company in a quasi-adjudicatory process; and in none did the intervention continue after the member could reasonably have been expected to know that company's intentions were questionable, if not illegal.

17. Admittedly, the three senators not from Arizona could claim that they were acting for the constituents in their own states who would benefit from Keating's businesses. But this ceases to be constituent service and should, like legislative activity, be evaluated from a broader perspective that takes into account all of a representative's constituents. From this perspective, the benefits that these senators were providing to some of their constituents arguably did not serve most others well, specifically taxpayers and depositors.

18. Relying on constituents to call attention to administrative abuses (what has been called "fire alarm" oversight) is said to be more common and more efficient than direct and continuous monitoring by Congress, called "police patrol" oversight (McCubbins and Schwartz 1984). But on the limitations of this and other forms of retrospective monitoring, see McCubbins, Noll, and Weingast 1987.

19. Cf. Kappel 1989; Rosenberg 1990.

20. Consider the standards practiced in Cranston's office, as described in the testimony of his aide, Carolyn Jordan: "Unless you have a complete kook, . . . the number one rule of this game is you never kiss a constituent off. That's the rule in our office. And you never tell them, no, unless they're asking you to do something that is just so far from the beaten path" (U.S. Senate 1991b, *Additional Views*, 84).

21. Many of the opinions in the line of cases interpreting the Administrative Procedures Act (*Pillsbury* and its progeny) are especially critical of secret congressional interventions, even when the courts do not invalidate the agency results. See Kappel 1989, 144–47.

22. U.S. Senate 1991a, pt. 4, December 14, 1990, p. 178.

23. Generally, see Lowenstein 1985, 808–9, esp. n. 86–87; Noonan 1984, 621–51, 687–90, 696–97. See also the opinions of

Justices Brennan and White in *U.S.* v. *Brewster* 1977, 558. In the most recent case dealing with public corruption, the best that the Supreme Court could do to justify sustaining a narrow standard (requiring explicit promises) was to say that a broader standard would "open to prosecution. . . conduct that in a very real sense is unavoidable so long as election campaigns are financed by private contributions or expenditures, as they have been from the beginning of the nation" (*McCormick* v. *U.S.* 1991, 1816).

24. The small academic literature on the subject has tended to be critical of the standard (Morgan 1992; Roberts 1985, 177–89; but see also Kappel 1989, 154–71). A helpful (and rare) discussion of the appearance standard by a contemporary philosopher is Driver 1992.

25. The potential effect on the conduct of others is the principal reason that Thomist ethics has traditionally treated appearing to do wrong under certain conditions as a distinct wrong. The wrong is called "giving scandal" and is defined as providing the "occasion for another's fall." It is considered a sin if one's otherwise permissible action is of the kind that is in itself conducive to sin, and it remains sinful whether or not one intends it to have any effect on others (Aquinas 1972, vol. 35, 109–37).

26. Some further specification of what should count as a general political orientation or a salient issue may be necessary in some circumstances, but it should be clear that agreement on the value of constituent service itself (the principle on which Keating and the Keating Five evidently most strongly agreed) is not sufficient. Constituent service, as has been suggested, may itself undermine the democratic process. Furthermore, in this case it seems plausible to conclude on the basis of his actions that Keating was less interested in constituent service justified as a general practice for all citizens than in the specific services provided for one constituent.

27. Lowenstein proposes a similar distinction between contributions "intended to influence official conduct and accepted with the knowledge that they are so intended" and those "intended solely to help the candidate get elected" (1985, 847). The distinction drawn in the text differs in at least two respects: (1) it takes the function, rather than actual intentions, as the criterion; and (2) it treats elections as only one of the relevant parts of the political process. Noonan also distinguishes contributions from bribes (1984, 696–97); but the two characteristics he regards as critical—size and secrecy—are better interpreted as indicators of the more basic distinction made in the text. If a contribution is small relative not to the total contributions but to other contributions a candidate receives and if it is public (or if the pattern of which it is a part is made public), then the contribution could be more plausibly seen as support for the candidate, rather than an attempt to influence official conduct.

28. This may be part of the rationale underlying the recent Supreme Court decision upholding a law that places limits on the ability of corporations to make independent expenditures on behalf of political candidates (*Austin* v. *Michigan Chamber of Commerce* 1990). Wealth accumulated by a corporation has "little or no correlation to the public's support for the corporation's political ideas" and therefore has "corrosive and distorting" effects on the political process (p. 1397). The idea is presumably that though the corporation is using the money to express substantive political views, the corporation's ability to do so does not derive from any substantive political support; its economic success ought not be translated so directly into political influence. See also Taylor 1991 and, more generally, Stark 1992.

29. An example of such a standard—already followed by some members—would be a rule requiring separation of the fund-raising from other functions in the offices of members (see U.S. Senate 1991b, *Additional Views*, 102).

30. A public official may act more directly than did the senators in this case. For example, when inserting a tax break for a particular company or individual in legislation, a member is more plausibly regarded as the direct agent than is the Internal Revenue Service. However, the corruption may still count as largely mediated because the other elements of corruption (the gain and the gain-benefit connection) continue to be mediated through the political process.

31. For a valuable analysis of the relation of deliberation specifically to problems of political finance, see Beitz 1989, 192–213.

32. Montesquieu, who among traditional political theorists most explicitly discusses corruption in branches of government, writes that a state will "perish when its legislative power becomes more corrupt than its executive" (1949–51, vol. 2, p. 407). More generally, see his "Corruption of Principle in the Three Governments" (vol. 2, 349–66).

33. For criticisms and further citations to the "functionalist" literature, see Rose-Ackerman 1978, 88–92; cf. Friedrich 1972, pt. 3.

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# INCOMPLETE INFORMATION AND IDEOLOGICAL EXPLANATIONS OF PLATFORM DIVERGENCE

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*One of the paradoxes of formal spatial voting models is the robustness of the theoretical result that candidates will converge toward centrists positions and the empirical observation of persistent policy divergence of candidates. A solution is that candidates are ideological (have policy preferences). When candidates have policy preferences and incomplete information about voter preferences, then platform divergence is theoretically predicted. Experimental tests of the ideological model are presented. It is shown that platform divergence is significant when candidates are ideological and have incomplete information about voter preferences. However, candidate positions are more convergent, on average, than the theory predicts, suggesting that subjects value winning independently of the expected payment.*

Most formal models of majority rule voting with two parties or candidates have generally assumed that the actors (either candidates or parties) are entirely motivated by the desire to win election.[1] The modeling approach of assuming purely winning-focused candidates is often called Downsian after the seminal work of Downs (1957). The empirical prediction of the Downsian model is that the parties or candidates will converge in policy positions if a unique equilibrium exists. Under the knife-edge case of complete information assumptions, the convergent position will be at the ideal point of the median voter (Hinich 1977). If no unique equilibrium exists (e.g., because preferences are not nicely behaved), then recent theoretical work suggests that converged positions are still likely (Tovey 1991).

Policy convergence, or the "median voter" result, has been used in many economic models to describe governmental behavior. In public economics, the use of the median voter conclusion is so prevalent that it is presented in most undergraduate text books (e.g., Browning and Browning 1987; Fisher 1987; Stiglitz 1988). In macroeconomics, the Downsian model serves as the basis for political business cycle and budget cycle models (Nordhaus 1975; Persson and Tabellini 1990; Rogoff 1990; and Rogoff and Sibert 1988).

A number of experimental studies have investigated the two-candidate convergence result. A recent review of the experimental literature can be found in McKelvey and Ordeshook 1990. Three different basic types of experiments have been run: (1) repeated elections with pure-strategy equilibria in two dimensions with perfect information (McKelvey and Ordeshook 1982); (2) repeated elections with pure-strategy equilibria in one and two dimensions with incomplete information (Collier et al. 1987; McKelvey and Ordeshook 1985a, 1985b; Plott 1991; Williams 1991); and (3) repeated elections without pure-strategy equilibria in two dimensions with perfect information (McKelvey and Ordershook 1982). The results of these experiments are largely supportive of Downsian policy convergence. In many cases, full convergence to the median occurs, even when information is incomplete at the beginning of the experiment. Even in the experiments with no equilibria, convergence increases with the number of elections and the candidate positions taken are largely within the Pareto-optimal, centrist set.

The theoretical and experimental work using the traditional Downsian framework then implies that we should not observe significant and persisting party or candidate differences. Nevertheless, much nonexperimental work demonstrates that substantial polarization of parties and candidates does exist in the United States and that these differences have survived over time (Grofman, Griffin, and Glazer 1990; Jung, Kenny, and Lott 1991; Peltzman 1984; Poole 1989; Poole and Rosenthal 1991). The nonexperimental work suggests that the traditional Downsian model is missing some significant aspect of political behavior. One solution that has been offered to explain the empirical dilemma is the incorporation of ideological candidates or party members in the Downsian model.[2]

The ideological-theoretical approach that has been suggested to explain platform divergence consists of assuming that candidates have policy preferences that are distinct or that ideological extremes have a greater influence in party decisions than moderates (Aldrich 1983; Alesina 1988; Aranson and Ordeshook 1972; Brady, Brody, and Ferejohn 1989; Calvert 1985; Fiorina 1974; Page 1978; and Wittman 1977, 1983, 1991). Page (1978) has labeled this the "party cleavage" theory of party platform determination. This later work builds on the party responsibility concept, in which parties are perceived as presenting two alternative ideological positions preferred by different groups of the electorate, giving voters a "choice," rather than an "echo," as Page notes.[3]

The potential influence of the policy preferences of candidates or party members in a general equilibrium formal spatial model is explored in the analysis of

Wittman (1977, 1983, 1991) in which divergent equilibria are presented. However, Calvert (1985) proves that if candidates have full information on voter preferences, then convergence of platforms is likely. Calvert also shows that when voter preferences are unknown, divergence is possible. Divergence with ideological candidates, then, crucially depends upon the extent of candidate knowledge of voter preferences.

The concept that candidates or parties may be ideologically motivated has had two noteworthy applications in the economics and political science literatures: (1) as an explanation of congressional voting behavior and (2) as an explanation for macroeconomic policy changes. An important focus of some recent nonexperimental work on voting behavior of members of Congress has been how far ideological preferences, instead of constituent interests, explain congressional votes (Kalt 1981; Kalt and Zupan 1984, 1990; Kau, Keenan, and Rubin 1982; Kau and Rubin 1979; Mitchell 1979; Peltzman 1984). While most of these studies find some evidence of ideological biases, they have difficulty in conclusively demonstrating the existence of such ideological forces. This is because the tests require estimating both constituent and candidate ideological preferences. They also often use votes on particular measures and ignore the difficulty of accounting for strategic behavior of legislators in voting.

The second application of the policy preference model is in macroeconomic theory. The seminal work on the partisan model of macroeconomic policy is Hibbs 1977.[4] Electorally induced policy cycles raise concerns about the design of democratic governments and the allowance for discretion of elected officials in implementing macroeconomic policies. If elections lead to an increased variability in business cycles that is not desired by the voters, then perhaps the political institutions should be altered. The likelihood, severity, and cause of electorally induced cycles is, as a consequence, a significant issue. Nonexperimental examinations of partisan effects in macroeconomic policies show that such effects are quite robust (Alesina, Cohen, and Roubini 1992; Alesina, Londregan, and Rosenthal 1991; Alesina and Rosenthal 1989; Husted Morton, and Waller 1992). This suggests that the ideological model does have validity.

Surprisingly, there are no published experiments on elections with candidates with policy preferences; nor am I aware of any unpublished work. Using experimental evidence, I shall examine the hypothesis that ideology explains platform divergence when candidates have incomplete information about voter preferences. First, a baseline experiment is reported on in which ideological candidates are told the position of the median voter. The results from the baseline are compared with two other experiments in which the median voter's ideal point is drawn randomly before each election. The experimental evidence demonstrates that candidate positions do not converge as much when candidates have incomplete information about voter preferences as with complete information and/or probability-maximizing candidates—as the theory predicted.

However, the candidate positions, while clearly more divergent than in the baseline experiment, in general converge more than the theory predicts suggesting that the subjects valued winning the elections in spite of the monetary rewards of the experiment. The data from these two experiments are used to estimate what value of winning best accounts for the chosen positions. The data suggest that the subjects placed a weight of approximately .68 on the expected payoffs from the experiment and .32 on winning, independent of the expected payoffs.

I report a fourth experiment in which candidates were ideological and the median voter ideal point was fixed (as in the baseline experiment) but in which candidates were not told the voter's ideal point. In this experiment, candidates initially chose positions quite similar in the first election to those chosen in the second and third experiments but converged significantly as candidates acquired information about the median voter's ideal point. Nevertheless, convergence was not as complete by the end of the experiment as in the baseline experiment. Assuming one-shot play, the optimal strategy for each subject in each election was computed given the information previously acquired; and this strategy was compared to actual behavior. The results demonstrate that subjects converged along a parallel path to that predicted given the information previously acquired but converged less than predicted, suggesting that subjects did not completely incorporate all the available information into their behavior.

## EXPERIMENTAL EVIDENCE ON IDEOLOGICAL CANDIDATES AND PLATFORM DIVERGENCE

### The General Experimental Design

I conducted four experiments on candidates with policy preferences in the fall of 1991 and 1992. These experiments will be labeled 1, 2, 3, and 4. The subjects were undergraduates recruited from political science courses required of all undergraduates at Texas A&M University. Some subjects were also recruited from notices posted in the student employment office. In all four treatments presented here, the candidates were "pure ideologues"; that is, the payments received by the candidates were solely based upon the policies enacted by the winning candidate. Candidates did not receive any additional payment for winning. The issue space was unidimensional, and voters and candidates had standard symmetric concave payoff functions over the issue space. The candidate payoff functions were distinct and centered about different "ideal points." The candidates were constrained to enact the policies advocated before election.[5] Because candidates are so constrained, they do "care" about winning, because without winning, they cannot implement their desired policies. If candidates are not constrained, then, as argued by Alesina (1987), candidates have an incentive in one-shot electoral games to diverge to their preferred positions once elected.

The voters in the experiments were artificial actors who simply voted for the candidate closest to their ideal point.[6] Since each election was a two-candidate race and candidates were constrained to enact the proposed policies, sincere and strategic voting are equivalent. Candidates were informed that the voters were artificial and that each voter would vote for the candidate who chose an issue position closest to the voter's ideal point. If candidates offered equivalent positions (or positions that implied equivalent voter payoffs), a voter would toss a fair coin with heads equal to a vote for candidate A and tails equal to a vote for candidate B.

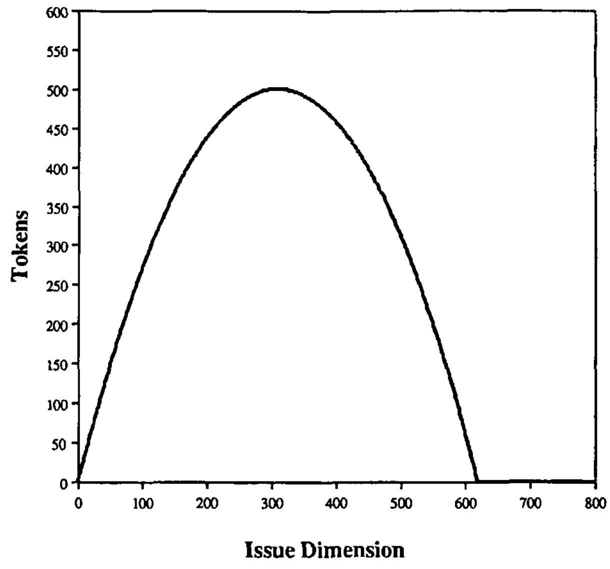
The subjects were divided into two groups, half designated as candidate A, half as candidate B. In each election, each subject was matched with a different member of the other group. Subjects were told that they always had a different opponent but were not told the identity of their opponents.[7] Candidate A's ideal point in the issue space was at 310 and candidate B's was at 490. The typical functional form for the candidate payoff functions was as follows:

$$U_i(x_w) = \begin{cases} 500 - k(x_i^* - x_w)^2 & \text{if } 500 - k(x_i^* - x_w)^2 \geq 0 \\ 0 & \text{if } 500 - k(x_i^* - x_w)^2 < 0, \end{cases}$$

where $U_i(x_j)$ is candidate i's payment when $x_j$ is the winning position, k is a positive constant less than one,[8] $x_i^*$ is candidate i's most preferred position, and $x_w$ is the position taken by the winning candidate. All candidates were given tables and graphs showing the shapes of the candidate payoff functions for both candidates. Figure 1 is an example of the one used for candidate A in experiment 2. Candidates were told that they could choose any real number as an issue promise; they were not constrained to the numbers on the table or to integers.[9]

**FIGURE 1**

**Candidate A's Payoffs in Tokens in Experiment 2**



If a candidate's ideal point was chosen by the winning candidate, that subject would receive five hundred tokens for that election period. Note that even a losing candidate could conceivably receive the maximum payoff if the opponent chose the loser's ideal point. The exchange rate of tokens was set such that the theoretical maximum that any subject could receive in an experiment was twenty dollars. Since the treatments consisted of different numbers of election periods (nine in experiments 1, 2, and 4, eight in experiment 3), the exchange rate was not the same, and tokens were used for expositional clarity only. While subjects were not told exactly how many elections were to be held, they were told an approximate time that the experiment would end and were able to estimate when the last election would be held. Since, in each election, each subject faced a different opponent and the winner was always constrained to enact proposed policies, there was no reason to expect that the last election should be significantly different from the first. Appendix A presents the general experiment instructions for experiment 3. The other experiments were simple variations of this format.

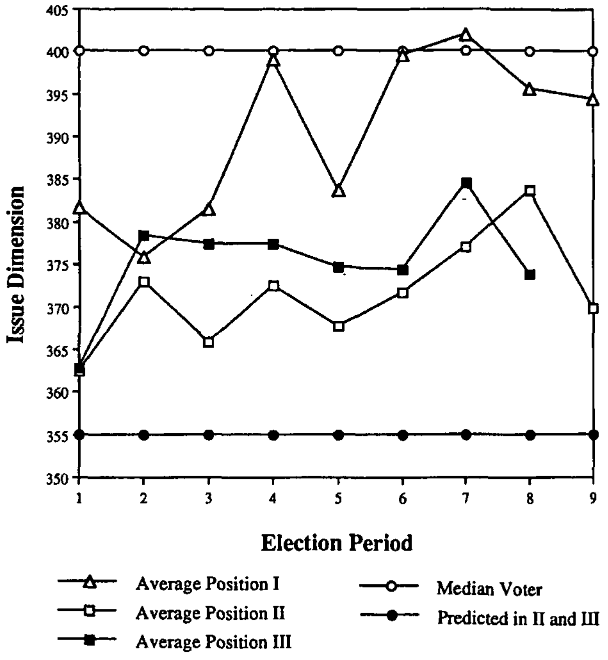
### Experiment 1: Ideological Candidates with Complete Information on Voter Preferences

As noted, Calvert (1985) demonstrates that in a one-shot electoral game with commitment by candidates to proposals and full information on voter preferences, the theory predicts convergence of platforms. Experiment 1 examines this proposition. Twenty subjects were used as candidates in the experiment. The voter preference function was fixed, and the voter's ideal point was set at 400. Candidates were given a table and a graph showing the voter's payoff function. Since the median voter's ideal point is 400, the expected payment is highest for both candidates at 400. For example, if A chooses 398 and B chooses 401, then B will win with certainty, A will receive 372.21 tokens (k = 1/64.8 in experiment 1), and B will receive 377.76 tokens. Clearly, A will find it optimal to move to at least 399 or higher. If A chooses 399 and B is at 401, then the expected payment for each is 374.98 (the voter will randomize). If A chooses 400, he will win for sure and receive a sure payment of 375, which is preferable to the expected payoff at the nonconvergent point. B will have the same incentive to move to 400. (Of course, if B knows that A has converged, then B will be indifferent.)

The positions taken by the candidates are summarized by election period as Average Position I in Figure 2. In the figure and statistical analysis pre-

FIGURE 2

**Average Positions in Experiments 1, 2, and 3**



sented in Table 1, candidate B positions are transformed into candidate A positions. Basic statistics on the data are reported in Table B-1 in Appendix B. If the B position is greater than 400, the transformed B position is 400 − (B − 400); and if the B position is less than or equal to 400, the transformed B positions is 400 + (400 − B).[10] The mean of the A and transformed B positions in experiment 1 is 390.34. There is some evidence that the positions converged over time. For example, assume that the observed positions are a function of the election period such that errors in position taking are decreasing with time as follows:

$$\text{position}_{it} = \alpha + \frac{\beta}{t_{it}} + \mu_{it}. \quad (2)$$

Then,

$$\lim_{t \to \infty} E(\text{position} \mid t) = \alpha.$$

The regression results estimating equation 2 for experiment 1 are reported in Table 1. The results indicate some convergence over time in positions with the predicted long-run position of 396.97.

### Experiments 2 and 3: Ideological Candidates with Probabilistic Voting

Under the assumption that ideological candidates with incomplete information about voter preferences are constrained to enact policy proposals, the theory predicts platform divergence. This prediction is considered in experiments 2 and 3. Voter preferences were probabilistic. Voting is modeled as probabilistic when random factors influence either actual voting or candidate perceptions of voter decisions.[11] In very simple terms, probabilistic voting is when voter preferences change or appear to change with each election, unlike the complete information set up in experiment 1, in which voter preferences are constant. In experiments 2 and 3, the voter preference function was similarly shaped as in experiment 1, with the ideal point of the voter randomly drawn from 310 to 490, inclusive, prior to each election. Candidates were told that the voter's ideal point was drawn randomly for each election. Voters were programmed actors as in experiment 1. Consider the game played by the candidates in each election period as a one-shot incomplete information game. If we assume that the candidates adopt Nash behavior, that is, each chooses a policy position to maximize the expected payoff given the other candidate's position, then in the simple functions used in the experiment, the equilibrium policy position for candidate A will be at 355 and for candidate B at 445. Note that this equilibrium prediction does not depend upon the value of k.

Figure 2 also presents the average candidate positions for experiments 2 and 3. Twenty subjects were used in experiment 2, 22 in experiment 3. Experiment 3 was run for only eight election periods due to a time constraint. On average, the candidates choose positions between the predicted optimal position and the median voter position. The mean position of A's and transformed B's was 371.49 in experiment 2 and 375.44 in experiment 3. Equation 2 was estimated using data from experiments 2 and 3; the results are also reported in Table 1. There is less evidence that the positions converged over time as in experiment 1. The predicted long-run positions in experiments 2 and 3 are 375.68 and 380.66, respectively. Divergence of platforms appears to occur, but the platforms converge more than the theory predicts. I shall consider two possible explanations for the greater-than-predicted convergence: (1) an insignificant monetary

TABLE 1

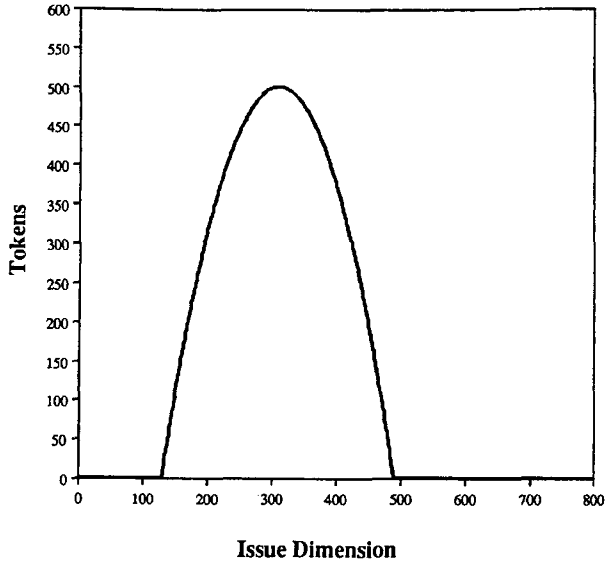
**Simple Estimation with Position as Dependent Variable**

| REGRESSION ESTIMATES | EXPERIMENT 1 | EXPERIMENT 2 | EXPERIMENT 3 | EXPERIMENT 4 |
|---|---|---|---|---|
| Intercept | 396.97*<br>(4.57) | 375.68*<br>(4.48) | 380.66*<br>(4.83) | 391.59*<br>(2.76) |
| 1/(elec. no.) | −21.09*<br>(11.05) | −13.33<br>(10.84) | −15.37<br>(11.05) | −27.21**<br>(6.67) |
| N | 180 | 180 | 176 | 216 |

*Note*: Standard errors are in parentheses.
$^*p \leq .10$.
$^{**}p \leq .05$.

FIGURE 3

**Candidate A's Payoffs in Tokens in Experiments 1, 3, and 4**



incentive for divergence and (2) subjects' desiring to "win" in spite of the monetary penalty.

*Effect of Increasing the Monetary Penalty of Convergence.* A plausible justification for the result of greater convergence than predicted is that the expected monetary amounts that the subjects lost by choosing more convergent positions were not significant enough to induce divergent position taking. In order to test this hypothesis, the payoff functions faced by the candidates in experiment 3 were made much steeper than in experiment 2 (by changing the value of k). Compare Figures 1 and 3. Nevertheless, as the equilibrium results just mentioned suggest, the results in experiment 3 are extremely similar to those in experiment 2 (see Figure 2). The mean A- and transformed B-position in experiment 3 is not significantly different from the mean in experiment 2. Again, candidates converge, on average, more than predicted by theory, even when the loss in monetary payment amounted to several dollars. (The average payment to subjects in experiment 3 was several dollars less than in experiment 2.)

*Candidates Motivated by Winning.* An alternative explanation for the greater-than-predicted convergence is that the subjects placed some value on being elected, independent of the expected monetary payment they received from the winning candidate's policy position; that is, the subjects demonstrated that they were willing to trade some loss in monetary value for choosing positions closer to the expected voter's ideal point in order to increase the likelihood of winning. As shown by Calvert (1985), when candidates care about winning independent of policy, there is more convergence of positions.

The game between the candidates with mixed motives can be modeled as one of incomplete information in which all candidates may be of either of two types: type I candidates, who are Wittman–Calvert policy-minded and maximize their expected payoffs from policy, and type II candidates, who are Downsian probability-of-winning maximizers, ignoring their payoffs from policy. Assume that nature draws candidate types. The probability that any candidate is type I is assumed to equal r, with the probability of being a type II equal to (1 − r). Thus, r is the proportion of type I potential candidates in the population. Assume that $0 \leq r \leq 1$. Alternatively, (1 − r) could be thought of as the weight that each candidate places on winning, independent of the expected payment derived from policy. Let $q_i = 1$ if candidate i is type I and $q_i = 0$ if candidate i is type II. The candidate-type-contingent objective functions, $V_a$, and $V_b$, can be expressed as follows:

$$V_a(q_a) = q_a[PU_a(x_a) + (1 - P)U_a(x_b)] + (1 - q_a)P$$

$$V_b(q_b) = q_b[PU_b(x_a) + (1 - P)U_b(x_b)] + (1 - q_b)(1 - P),$$

where $P$ is the probability of candidate A's winning the election, given that the median voter's ideal point is drawn randomly from 310 to 490, inclusive. If we assume that each candidate has some prior on the probability that the opponent is a type I, we can complete the information structure and solve the game for the Bayesian–Nash equilibrium.

For example, suppose that r is common knowledge; everyone knows the true proportion of type I candidates.[6] Consider, first, candidate A's choice problem. For the equilibrium to be Bayesian–Nash, A's choice of $x_a$ must be a best reply to candidate B's choice of $x_b$. Let $X_i(q_i)$ be candidate i's optimal reply, given his type. While candidate A is aware of his own type, he is uncertain as to what type of candidate B he faces. Candidate A's objective function depends on his beliefs about candidate B's type, as follows:

$$E[V_a(x_a, \bar{X}_b(q_b))] = rV_a(x_a, \bar{X}_b(1)) + (1 - r)V_a(x_a, \bar{X}_b(0)),$$

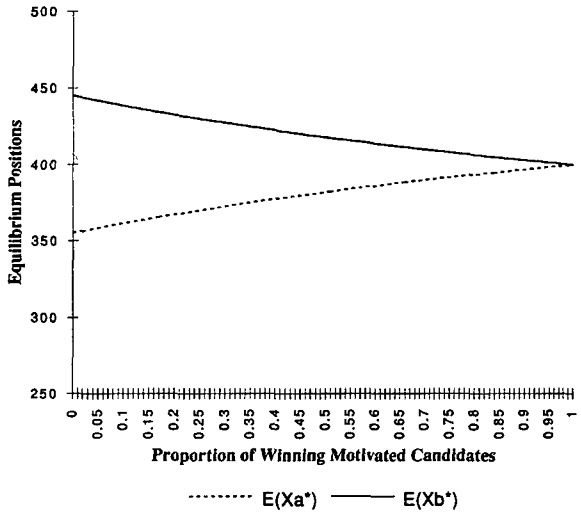
where $\bar{X}_b(q_b)$ is candidate A's expectation of B's position, given B's type. Note that candidate B's expectation of A's position, given A's type, is equal to $\bar{X}_a(q_a)$. If candidate A is type I, then he will choose the $x_a$ that maximizes his expected payoff given his expectation of candidate B's type-contingent strategy:

$$X_a|_{q_a = 1} = (\tfrac{1}{3})[r\bar{X}_b(1) + (1 - r)\bar{X}_b(0) + 620].$$

If candidate A is type II, then he will choose the $x_a$ that maximizes his probability of winning, given his expectation of candidate B's type-contingent strategy. A will wish to be as close to candidate B as possible but, at the same time, closer to the expected median voter position, 400. If we define $\varepsilon_a$ as a positive real number such that $0 < \varepsilon_a < |400 - [r\bar{X}_b(1) + (1 -$

FIGURE 4

**Expected Positions with Mixed Motives**



$r)\bar{X}_b(0)]|$, then a type II candidate A's optimal behavior is

$$X_a|_{q_a=0} = \begin{cases} r\bar{X}_b(1) + (1-r)\bar{X}_b(0) + \varepsilon_a \\ r\bar{X}_b(1) + (1-r)\bar{X}_b(0) \\ r\bar{X}_b(1) + (1-r)\bar{X}_b(0) - \varepsilon_a \end{cases}$$

$$\begin{aligned} &\text{if } r\bar{X}_b(1) + (1-r)\bar{X}_b(0) \le 400 - \varepsilon_a \\ &\text{if } r\bar{X}_b(1) + (1-r)\bar{X}_b(0) = 400 \\ &\text{if } r\bar{X}_b(1) + (1-r)\bar{X}_b(0) \ge 400 + \varepsilon_a. \end{aligned}$$

Clearly, in the limit as $\varepsilon_a$ approaches zero, the optimal response for a type II candidate A is to choose $x_a = r\bar{X}_b(1) + (1-r)\bar{X}_b(0)$. For the remainder of the analysis, we will evaluate the optimal strategy for type II candidates A and B in the limit as each $\varepsilon_i$ approaches zero.

Candidate B's optimal behavior can be similarly derived. We have now specified the optimal strategies of the two candidates as functions of their types, their beliefs about the type of their opponents, and their expectations about their opponents type-contingent strategies. The Bayesian-Nash equilibrium positions of the two candidates is simple to derive. Define $E(X_i)$ as the expected Bayesian-Nash position of candidate i. Thus, $E(X_i) = rX_i(1) + (1-r)X_i(0)$. By solving simultaneously it is straightforward to show that

$$E(X_a) = \frac{490r - 1{,}200}{r - 3}$$

$$E(X_b) = \frac{310r - 1{,}200}{r - 3}.$$

The expected equilibrium positions as a function of (1 − r) are shown in Figure 4. Note that when r equals 1, as expected, the Bayesian-Nash positions are 355 for candidate A and 445 for candidate B. As (1 − r) increases, the probability of either candidate's being Downsian increases; and the expected equilibrium positions converge toward the expected median voter position of 400. Complete convergence occurs only when r equals zero.

Using the data from experiments 2 and 3, the population parameter r can be estimated from the estimated expected candidate positions; that is, given the expected values of the candidate positions in experiments 2 and 3, the estimated values of r are .72 and .64, respectively. The data, then, suggest that 28–36% of the subjects were motivated primarily with choosing winning positions under the assumptions of the analysis. Alternatively, we could think of the subjects as placing a weight of approximately .68 on the expected payoff from the experiment and a weight of .32 on winning election.

## INFORMATION REVELATION AND THE CONVERGENCE OF PLATFORMS

The contrast of results in experiment 1 versus experiments 2 and 3 suggest that the extent of information that candidates have about the location of voter preferences is a crucial determinant how far ideological preferences cause platform divergence. In experiment 4, the relationship between divergence and candidate knowledge is explored further. In this experiment, the voter's ideal point was fixed as in experiment 1, but the candidates were not told the location of the voter's ideal point. Candidates were told that the voter's ideal point was somewhere between 310 and 490, inclusive. They were also told that the voter's ideal point would remain unchanged throughout the experiment.

Twenty-four subjects were used in the experiment. Because of the random matching process and the aforementioned knowledge that the voters would always vote sincerely, as the number of elections increased, candidates conceivably gained information on the location of the voter's ideal point. We can calculate the information gained by subject by assuming that each subject did the appropriate Bayesian updating (truncation of the uniform) after each election. For example, suppose that both candidates A and B choose the Nash equilibrium positions of 355 and 445 in the first election. The voter will be indifferent between the two, and a fair coin will be tossed designating the winner. Suppose A wins. Both candidate A and B now can revise their upper bound on the voter's ideal point from 490 to 400, since the median voter's ideal must be less than candidate B's position (otherwise B would have won) and at least as close to candidate A's as it is to candidate B's. However neither A nor B will be able to revise their lower bound of 310, since it is conceivable that the voter's ideal point is less than candidate A's position. If we assume that each election is a one-shot game, given the random matching procedure, then the

FIGURE 5

**Average Position Versus Predicted Position in Experiment 4**

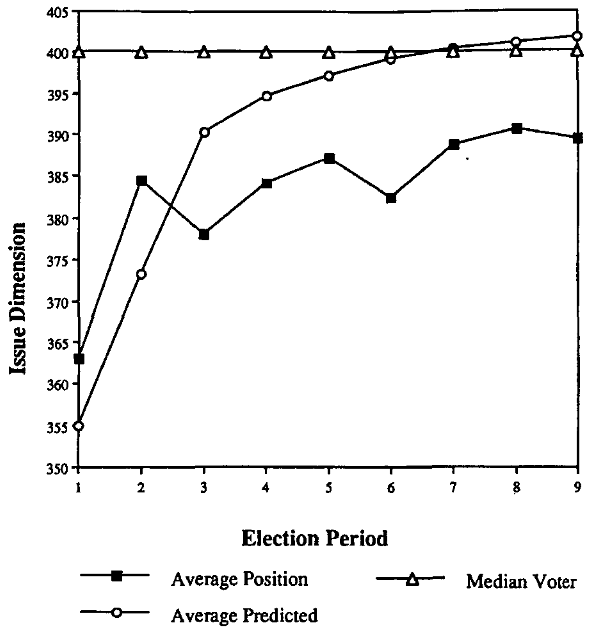


TABLE 2

**Dependent Variable: Candidate Position in Experiment 4**

| VARIABLE | ESTIMATE | STANDARD ERROR |
|---|---|---|
| Constant | 362.96** | 5.43 |
| Election | | |
| 2 | 21.40* | 7.68 |
| 3 | −6.35 | 7.68 |
| 4 | 6.09 | 7.68 |
| 5 | 2.98 | 7.68 |
| 6 | −4.72 | 7.68 |
| 7 | 6.31 | 7.68 |
| 8 | 1.84 | 7.68 |
| 9 | −1.18 | 7.68 |

*Note:* In contrast to Table 1, dummy variables were used to represent the election periods; i.e., the dummy variable for election 2 takes on a value of 0 in election 1 and 1 in elections 2 and higher, etc. Thus, the intercept term in this regression is the expected position taken in the first election period while the intercept terms in the regressions reported on in Table 1 are estimated long-run positions as the number of election periods approaches infinity.

$^{*}p \le .10$.

$^{**}p \le .05$.

predicted equilibrium position of a candidate in any election will be a function of the new information acquired in the previous elections, and the candidate's prior on the information possessed by the opponent.

Assume that candidate A believes that he cannot predict the extent of candidate B's acquired knowledge and assumes that candidate B acts as if the voter's ideal point lies between 310 and 490, inclusive. Assume that candidate A also believes that candidate B has the same prediction about A's knowledge of the voter's ideal point. Then candidate A's optimal strategy, given his beliefs, is to maximize the expected payoff assuming that the opponent will choose the Nash equilibrium position of 445. Under these assumptions, predicted positions for each subject can be computed, given the information revealed to the subjects in previous elections. In Figure 5, the actual average positions of the candidates in experiment 4 are presented, with the average predicted positions of the candidates, given the information they have acquired.

The predicted positions converge significantly in the first few election periods, reflecting the information revealed to the subjects in the previous elections. Notice that the actual average position taken in the first election period is quite close to the average position in the first election period in experiments 2 and 3, while in the remaining periods, the average position in experiment 1 is more convergent. As reported in Table 1, estimation of equation 2 for experiment 4 demonstrates significant convergence of positions over time with a predicted long-run position of 391.59. A simple, linear step regression of the policy position taken by a candidate as a function of the election period presented in Table 2 demonstrates that the convergence happens fairly quickly, as predicted. The only significant independent variable is the second period. However, the initial uncertainty about voter preferences does cause less convergence over time as in experiment 1. The evidence suggests that subjects do not fully incorporate all the information available to them through previous experience (i.e., they do not converge as much as in experiment 1); but the acquisition of information does lead to more convergence over time than is evidenced in the probabilistic voting environments of experiments 2 and 3. Experiment 4, then, supports the Calvert result that as the amount of information about voter preferences increases, ideological candidates are more likely to converge. Again, the importance of candidate uncertainty over voter preferences as a determinant of platform divergence when candidates are ideological is evident.

## CONCLUSION

The ideological model of candidate behavior is becoming increasingly popular as an explanation of partisan differences. The ideological framework seems to have empirical validity, since it can explain observed platform divergence, unlike the traditional Downsian model. It is now receiving considerable attention in models of macroeconomic policy. In macroeconomics, partisan differences caused by ideology have been posited to be one possible explanation of the magnitude of business cycles in democratic countries.

The results of the experiments reported here validate the ideological explanation of platform divergence and underline the importance of incomplete information in determining the extent of ideologically induced platform divergence. When ideological candidates have incomplete information on voter preferences, divergence is significantly greater than when ideological candidates have complete information on voter preferences. Moreover, as candidates acquire information on voter preferences, convergence of platforms increases significantly.

The analysis presented here parallels the conclusions of Kollman, Miller, and Page (1992). Using computer simulations, they investigate the influence of party information over voter preferences under assumptions of boundedly rational adaptive parties choosing positions in multidimensional issue space. They also find that as party information over voter preferences increases, convergence is more likely, even when parties are ideological. Their results and mine, then, suggest that the roles of information and ideological preferences in determining party or candidate positions are worth further exploration. One avenue for future study is how far candidate discretion affects party and candidate platform divergence, once the candidate is in office. In the simple experiments reported here, the candidates were constrained to enact proposed positions. As noted, Alesina (1988) argues that unconstrained candidates will diverge to their ideal points once elected. Experiments involving unconstrained candidates and nonartificial voters might be useful examinations of the hypothesis that platform divergence is greater under these assumptions.

## APPENDIX A: EXPERIMENT 3 INSTRUCTIONS

I. What is this experiment about?
  A. This experiment is a study of elections.
  B. As subjects you will each be assigned to be a candidate. You will remain that candidate throughout the experiment.
    1. In this experiment you will be divided into two groups, A and B, each containing an equal number of subjects. Members of group A will be a candidate A throughout the election. Members of group B will be a candidate B throughout the experiment.
    2. In this experiment the voters are "programmed actors."
      a. That is, the voters are like robots. They are programmed to always vote for the candidate that promises them the higher payoff value.
      b. If two candidates offer the voter the same payoff value, the voter will flip a fair coin with heads equal to a vote for candidate A and tails equal to a vote for candidate B.
    3. After we go over the instructions, you will receive an information folder that assigns you to be either candidate A or candidate B.
    4. The information folder will also have other information that we will shortly discuss.
  C. One important rule of this experiment is that once we begin, *no one is allowed to talk or communicate in any way to anyone else.*
  D. *The only communication permitted is the communication that occurs in the information folders that you will be given. Failure to comply with this rule will result in the loss of your payment.*

II. What determines how much you will be paid?
  A. You will each be paid for your participation on the basis of the decisions that you make.
  B. If you are careful and make good decisions, you can make a substantial amount of money.
    1. However, the payoffs in the experiments are not necessarily fair.
    2. Your payoff depends partly on your decisions, partly on the decisions of others, and partly on chance.
  C. The payoffs will be given in tokens. The tokens will be exchanged for dollars at the end of the experiment.

III. How does the experiment work?
  A. The experiment is divided into a number of election periods, and each period consists of two steps.
  B. The first step consists of a campaign.
    1. First you will be matched with a subject from the *other* group. He or she will be the other candidate in your election.
    2. Note that all elections will be between a candidate A and a candidate B; members of the same group will not be matched with each other.
    3. Each of you will make a campaign promise to voters about your plans if you are elected.
    4. The other candidate in the election will also make a campaign promise to voters at the same time.
    5. The campaign promise you make will imply certain payoffs to voters, yourself, and the other candidate if you are elected. The payoff structure will be explained shortly.
  C. In the second stage, an election takes place.
    1. The voters vote for either you or the other candidate. Remember, a voter will always vote for the candidate that promises him or her the higher payoff. If you and the other candidate promise equivalent payoffs, the voter will flip a fair coin with heads equal to a vote for

candidate A and tails equal to a vote for candidate B.

2. You are then told who won the election and your payoff for that election.

D. After you are told your payoff another election will be held.

E. In each election, you will be matched with a different member of the other group. Thus, if you are a member of group A, in each election, you will be matched with a member of group B. If you are a member of group B, in each election, you will be matched with a member of group A.

F. We will hold elections so that you will be matched with each of the subjects of the other group *exactly* once or we run out of time, whichever comes earlier. So if you are a candidate A, you will be matched with each of the candidate Bs, at most, *exactly* once. If you are a candidate B, you will be matched with each of the candidate As, at most, *exactly* once. Quite possibly we will run out of time and you will not be matched with all the members in the other group.

G. Each subject will be given a sheet describing what has happened in his or her earlier elections in the experiment.

IV. How are the payoffs determined?

A. You can choose any number from 0 to 1000 as a campaign promise.

1. You can think of this number as a position or stance on a particular policy issue that both voters and candidates care about.
2. We will call this number your issue position number.
3. This number or issue position will imply a given payoff to each voter and candidate if you win.

B. The tables have the payoff schedules. The figures are the same payoff schedules graphed. [Each subject was given a copy of tables and graphs of the candidate payoff schedules. An overhead projector was also used to show the tables and graphs.]

1. Notice that a candidate can at most receive 500 tokens in one election.
2. Candidate A will receive 500 tokens if issue position number 310 is chosen. 310 is then a member of group A's "ideal" or most preferred issue position.
3. Candidate B will receive 500 tokens if issue position number 490 is chosen. 490 is then a member of group B's "ideal" or most preferred issue position. [These points were shown to subjects using the overhead.]
4. If, for example, issue position number 200 is chosen, a member of group A will receive 313.27 tokens, and a member of group B will receive 0 tokens. [These points were shown to subjects using the overhead.]
5. If issue position number 450 is chosen, a member of group A will receive 197.53 tokens, and a member of group B will receive 475.31 tokens. [These points were shown to subjects using the overhead.]

C. Note that you may receive some payoff even if you lose the election.

1. The payoff you receive depends *only* on the policy adopted by the winning candidate.
2. For example, suppose you are a member of group A and you lose the election. Suppose the other candidate chose issue position number 470. Then you will receive 104.94 tokens, even though you have lost. But if the other candidate chose 520, you will receive 0 tokens.
3. What if you win the election and you chose issue position number 470 yourself? Then you will receive 104.94 tokens also.
4. *You do not get an extra payoff for winning.*

D. You will get to keep this instructions folder with the candidate payoff schedules throughout the experiment. You may refresh your memory of these instructions and the shapes of the payoff schedules at any time during the experiment.

E. *The payoff schedules for the candidates will stay the same throughout the experiment.*

F. Voter payoff schedules are similar with some differences:

1. Voters also can receive at most 500 tokens.
2. The "ideal" or most preferred issue position for voters will be different in each election.
   a. The "ideal" issue position for voters will be drawn randomly for each election.
   b. It will be between 310 and 490, inclusive.
   c. That is, it will never be less than 310 or more than 490, but it may be *any* number between 310 and 490.
3. Prior to each campaign a new voter payoff schedule will be randomly selected.
4. You will *not* be told the voter payoff schedules either during the campaign or after the election. Only voters know their own payoff schedules.

V. Summary of the experimental process

A. You are told your group membership. You are then matched with a member of the other group. He or she will be the other candidate in the election.

B. The payoff schedule for voters for the next election is randomly selected and revealed to voters.
C. You and the other candidate choose issue positions to promise to voters.
D. Voters are told candidate promises.
E. Voters vote. Voters are required to choose the candidate that promises the highest payoff to themselves and to flip a coin if the payoffs are equal.
F. The results of the election are revealed to you and the other candidate.
G. You are matched with a *different* member of the other group.
H. The election process begins again and will continue either until you are matched with each member of the other group *exactly* once or we run out of time, whichever comes first.

## APPENDIX B

Table B-1 summarizes the descriptive statistics of the four experiments.

**TABLE B-1**

**Descriptive Statistics**

| STATISTIC | EXPERIMENT 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Mean | 390.34 | 371.49 | 375.44 | 383.03 |
| S.D. | 40.16 | 39.14 | 40.39 | 27.31 |
| Minimum | 265.00 | 230.00 | 274.35 | 310.00 |
| Maximum | 646.00 | 470.00 | 476.11 | 470.00 |

## Notes

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1. For a review and analysis of this literature, see Enelow and Hinich 1984; Mueller 1989, 179–216

2. There are a few majority voting models with nonideological candidates that yield equilibria with policy divergence. A full discussion of this literature is beyond my scope here. See Austen-Smith 1987; Bernhardt and Ingberman 1985; Cameron and Enelow n.d.; Palfrey 1984. See also my earlier work, Morton 1987.

3. For more details, see also Monroe 1983.

4. For recent work, see Alesina 1988; Alesina, Londregan, and Rosenthal 1991; Alesina and Rosenthal 1989; Havrilesky 1987; Hess 1991; Hibbs 1987.

5. This feature is modeled in Calvert 1985 and Wittman 1977, 1983.

6. This resembles McKelvey and Ordeshook 1982

7. In the McKelvey and Ordeshook's (1985a, 1985b) experiments, candidates repeatedly ran against the same candidates. The matching process was used in this analysis to avoid the potential of collusive behavior among the candidates.

8. In experiments 2, k equaled 1/64.8; and in experiments 1, 2, and 4, k equaled 1/192.2.

9. Examples were used in the presentation of the instructions that were not on the table and were not integers. Some subjects did use nonintegers as policy positions.

10. Means equality tests showed that the hypotheses that the means of the A and the transformed B positions in each experiment are equal could not be rejected.

11. More detailed discussions of probabilistic voting can be found in Coughlin 1991's Enelow and Hinich 1984; Mueller 1989.

12. Wood and I consider the equilibria in which beliefs are heterogeneous and estimate the distribution of beliefs econometrically Morton and Wood 1992.

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# THE POLITICAL ECONOMY OF ELECTIONS IN LATIN AMERICA, 1980-1991

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*The process of political democratization in Latin America during the 1980s created a rare opportunity to explore the political economy of elections outside of the North Atlantic basin. Using interrupted time-series analysis, I explore the impact of elections on macroeconomic performance in eight Latin American nations. The findings indicate that macroeconomic performance has fluctuated with the electoral calendar but that contrary to the traditional business cycle literature, as well as the conventional wisdom about Latin America, competitive elections have enhanced, rather than undermined, the capacity of political leaders to address outstanding problems of macroeconomic management. The analysis suggests that the relationship between democracy and economics is captured more adequately by a "political capital" model than by its traditional theoretical alternative.*

Do democratic political leaders attempt to manipulate the economy for electoral purposes? Do they succeed? Beginning with the pioneering work of Nordhaus (1975) and Tufte (1978), these questions have generated an extensive body of research with important implications for broader debates over democracy and economics. The accumulated evidence suggests that voters respond to the state of the economy, and the economy to shifts in party control (e.g., Alesina and Sachs 1988; Erikson 1989; Hibbs 1977, 1987); but comparatively few studies have sustained the traditional assumptions regarding the impact of electoral competition on macroeconomic policy or performance (Alt and Chrystal 1983).

A key limitation of existing research is its narrow comparative base. With few exceptions, the literature on the political business cycle has focused upon the experience of the major industrial democracies, particularly the United States--a country noted less for the economic capacities of state actors vis-à-vis private-sector interests than for the "privileged position of business" (Lindblom 1977). In comparative perspective, however, all of the industrial democracies stand out in ways that might be expected to undercut the theoretical linkages that have been drawn between elections and economic outcomes. Expertise, technocracy, and relatively autonomous institutions, such as the German Bundesbank, play a critical role in macroeconomic management, insulating the economy from political manipulation. Others have pointed to the mitigating effects of economic prosperity, educated electorates, independent media coverage of economic policy, extensive welfare systems, and institutionalized forms of consultation that lengthen the time horizons of affected social groups (Haggard and Kaufman 1989, 242). The importance of such constraints is difficult to assess, however, within a framework of comparison that is limited to the North Atlantic.

I shall reevaluate the political business cycle literature on the basis of the experience of a set of less industrialized nations. How applicable is the political business cycle model to policymaking contexts characterized by weak institutions, widespread poverty, and economic instability? Do elections in such contexts provoke recurring patterns of opportunistic policy choice and undermine economic performance? What factors condition the relationship between elections and macroeconomic management? A comparative study of Latin American democracies over the course of the past decade suggests that macroeconomic performance fluctuates in response to elections but that the relationship between democracy and economics is captured more adequately by a "political capital" model than by its traditional theoretical alternative. Contrary to both the political business cycle literature and the conventional wisdom about Latin democracy, competitive elections in the region have enhanced, not undermined, political leaders' capacity to address major problems of macroeconomic management.

## ELECTIONS AND ECONOMICS IN LATIN AMERICA

Perhaps nowhere in the world have questions about the economic impact of elections taken on greater significance than in Latin America. Runaway inflation, balance-of-payments crises, irregular growth, and other symptoms of macroeconomic instability have characterized the region throughout the twentieth century. The root of these problems has been traced back to underlying patterns of political culture, social structure, and/or international economic dependency. More proximately, however, leading theoretical frameworks have attributed causal significance to the tension between popularly based government and economic performance. The contributions of the literature on democratic breakdown and bureaucratic authoritarianism in Latin America center around this tension (O'Donnell 1973). Likewise, a central presumption of the literature on economic reform and liberalization has been "that the only choice available to Third World societies is between a 'hard', authoritarian state, which can impose the liberal order without the need for a political consensus, and a 'soft' populist state, which would achieve political solvency through distributional appeasement" (Banuri 1991, 13). The same theme recurs in more recent research (e.g., Dornbusch and Edwards 1991; Malloy 1987). Indeed, if anything, concern heightened over the tension between democracy and effective policy management in response to the developments of the 1980s.

During the past decade, Latin America underwent the longest and deepest wave of democratization in its entire history. Simultaneously, drastic reversals in international capital flows produced the sharpest decline in living standards since the Great Depression. By late 1990, the average annual rate of inflation for the region approached 1,500%, while per capita income had declined 10% relative to the early 1980s (Comisión Económica para América Latina, 1990, 26–27; CEPAL). As a result of these setbacks, most regional observers expressed skepticism about the capacity of elected leaders to consolidate competitive systems. The problem has not merely been conceptualized in terms of the destabilizing effects of economic crisis. The willingness and ability of democratic leaders to mount effective responses to economic challenges has also been questioned, particularly in the so-called new democracies. According to Ames for example, the "medicine of austerity" is unlikely to be imposed in these states: "Too many factors—weak politicians, frequent elections, mobilized populations, and weak party loyalties—encourage the formation of large coalitions, the avoidance of policies that impose costs on anyone, and a focus on short-term survival by politicians" (1987, 82; see also Hurtado 1989, 101–2; Seligson and Muller 1987, 322–23).

Hence, while prevailing economic wisdom has dictated politically unpalatable programs of stabilization and adjustment, electoral considerations have been seen as pushing democratic leaders in the opposite direction. In early 1991, for example, President Rodrigo Borja of Ecuador was portrayed as facing an uncomfortable dilemma—"to go down in history as the man who set Ecuador's economy on the 'right' track, or to ditch some of his economic policy objectives and try to improve his party's electoral chances" (*Latin American Weekly Report* 1991, 11). In a more general vein, the 1990 report of the Inter-American Dialogue suggested that a vicious circle had developed within the region:

> Democratically-elected leaders who have been unable to stem economic decline have lost support and authority, thereby making it even harder for them to institute and sustain the painful adjustments required for economic improvement. . . . This vicious cycle may condemn some Latin American nations to a long period of both economic hardship and political turbulence. . . . As one democratic leader after another loses support, the credibility of democratic rule itself may be endangered. (1990, 52)

In short, for Latin Americanists, the traditional political business cycle has been viewed less as an empirically tenuous model of economic management than as a real threat to regional economic and political stability.

Underlying the pessimistic assessments of Latin America's political future are a series of assumptions about democracy and economic policy:

1. The level of support for elected governments varies directly with their capacity to deliver short-term material payoffs to voters.
2. Democratic governments pursue policies that maximize electoral support.
3. Policies that maximize electoral support jeopardize long-term economic performance.

In the traditional business cycle literature, these assumptions about the relationship between democracy and economics were applied to the trade-off between price stability and full employment. As hypothesized by Nordhaus, the approach of elections leads incumbents to adopt policies that minimize unemployment (1975, 184). Immediately after elections, victors raise unemployment to combat inflation. Empirical support for this hypothesized cycle has remained weak, however, raising a series of questions about its underlying assumptions. Voters may not be myopic, nor policymakers willing to sacrifice economic performance for political gains, particularly if they are uncertain about their capacity to produce optimally timed policy results (Barry 1985; Lewis-Beck 1988).

The validity of the assumptions underlying the business cycle literature, however, presumably varies across national boundaries with the nature of the electorate, the political salience of short-term economic performance, the structure of the economy, the autonomy of economic policymakers, and political institutions—all of which may be expected to shape the anticipated costs and benefits of manipulating the economy to achieve political goals. Research discounting the existence of business cycles in the North Atlantic region thus fails to address directly the concerns that have been expressed about the capacity of Latin democracies to cope with economic challenges. The sophistication of voters, the complexity of the economy, the institutional constraints on policy formation, and the relative power of the private sector are all more limited in Latin America than in the United States or Western Europe, creating potentially greater incentives and opportunities for politically motivated macroeconomic management.

Thus, even if elections do not breed what Tufte called "a lurching, stop-and-go economy" in the United States or Western Europe (1978, 143) recurring patterns of opportunistic policy choice may well drive Latin American economies. Indeed, arguably the most theoretically telling context in which to assess the political business cycle literature is not the North

Atlantic but Latin America. If politically driven patterns of economic management cannot be identified in the latter region, despite its contrasts with the North Atlantic and despite the theoretical preoccupation of regional specialists with the tension between popular government and economic performance, the political business cycle literature is highly implausible. If, on the other hand, Latin elections can be linked with recurring patterns of disruptive policy management, the relevance of the existing body of research on the political business cycle would be significantly qualified, casting new light upon the interrelationships between economics and politics. It should be emphasized that from the perspective of countries grappling with unresolved problems of poverty, inequality, and political legitimacy, these interrelationships are of fundamental practical, as well as theoretical, importance. If democracy means that economic performance is sacrificed in order to maximize short-term political gains (as the traditional political business cycle literature suggests), major questions are raised about the capacity of competitive institutions to address the challenges of economic development throughout the less industrialized regions of the world.

Prior research on Latin America has failed to clarify the relationship between competitive elections and economic performance. Comparisons of democratic and authoritarian regimes provide no evidence that electoral competition undermines economic management (Remmer 1989, 1990). Nevertheless, Ames (1987) has presented data indicating that government spending rises both before and after elections in Latin America, except in those relatively rare instances in which incumbents managed to achieve reelection. These results, however, are difficult to interpret because they are based upon annual data for all Latin nations and fail to address the critical issue of macroeconomic performance. The finding that spending rises with elections is also not consistent with more detailed longitudinal studies of policy cycles in Latin democracies. Stallings' (1978) research on pre-1970 Chile, for example, identified a pattern whereby fiscal and credit policies expanded during the first two years of new administrations; that is, macroeconomic policies in Chile followed a cycle corresponding to electoral events, but the cycle worked in favor of the consolidation of support for new presidents, rather than the reelection of incumbents.

The applicability of prior Latin American research to the economic dilemmas and political concerns of the 1980s is also questionable. The democracies that emerged in the past decade were far more inclusionary than their predecessors, potentially augmenting popular pressures on political leaders. Comparisons with the past are further complicated by the legacy of the debt crisis, which simultaneously increased both the anticipated economic costs and political benefits of politically motivated manipulation of the economy. To begin with the issue of political benefits, available survey data for the 1980s indicate that voters throughout Latin America regarded the economy as their nation's most pressing problem (e.g., Catterberg 1989, 44; *Caretas* 1987, 21; *Caretas* 1989, 40; *Caretas* 1990, 16; Kuczynski 1990, 93). The state of the economy also affected electoral outcomes. After 1982, no governing party or coalition in the region managed to increase its share of the electoral vote; only two won reelection; and the magnitude of incumbent losses varied indirectly with short-term economic performance (Remmer 1991). The electoral salience of economic performance thus achieved high levels during the 1980s, creating strong incentives for policymakers to sacrifice economic prudence for political gains.

Concomitantly, however, the debt crisis raised the economic costs of overly expansive macroeconomic policies, crystallized the trade-offs between short- and long-term economic performance, and limited the capacity of political actors to effect economic improvements. By the mid-1980s, the debates that had once pitted structuralists against monetarists had been largely superceded by a new internationally defined and policed economic orthodoxy, which restricted the menu of policy choices more narrowly than at any time in the past (see Williamson 1990). To retain access to international financial markets, policymakers were pressed to sacrifice concerns for growth or equity in favor of programs of adjustment and stabilization emphasizing fiscal austerity, market liberalization, privatization, and shifts in resources to tradables production—all of which could be expected to run up against considerable political opposition from business and popular-sector forces alike, due to their immediate economic costs.

The Latin American experience of the past decade thus offers an unusual opportunity for reevaluating the traditional wisdom regarding elections and democracy. No set of countries outside of the Organization for Economic Cooperation and Development offers a more extensive series of elections with which to reevaluate the traditional political economy literature; the politics of the region differ from that of the North Atlantic in ways that might be expected to affect the incentives and opportunities for shortsighted manipulation of the economy; and the conditions associated with the debt crisis eliminated much of the usual opacity surrounding policy choice, thereby accentuating the dilemmas of policymakers and heightening the theoretical significance of their choices for the literature on democracy and economics. During the 1980s, voter sensitivity to economic performance in Latin America was perceptible and pronounced; the range of policy options, limited by the realities of the ongoing economic crisis; and the anticipated economic costs of enhancing short-term performance for political gain, high and relatively well defined. Moreover, the international roots of the constraints on policy choice presented leaders across the region with similar policy dilemmas, facilitating comparative analysis of differing national experiences.

## DATA AND METHODOLOGY

The subsequent analysis of the effects of elections on economic performance in Latin America begins with a longitudinal study of economic policy outcomes in eight South American countries and then proceeds in a more comparative direction in an attempt to account for variations through time and across cases. The study covers all competitive elections involving the transfer of power from one elected president to another during the 1980–91 period, with the exception of elections in the Caribbean Basin, where the effects of domestic policy choice were swamped by international conflict, guerrilla warfare, and massive infusions of U.S. aid. Explicitly excluded from consideration are noncompetitive elections and foundational elections involving the transfer of power from military to civilian rule, in which the incentive structure facing incumbents differs significantly from that presupposed by the business cycle literature. Fourteen elections in eight Latin American nations fit the specified criteria: Argentina 1989; Bolivia 1985 and 1989; Brazil 1989; Colombia 1982, 1986, and 1990; Ecuador 1984 and 1988; Peru 1985 and 1990; Uruguay 1989; and Venezuela 1983 and 1988.

To establish a basis for comparing the impact of elections in Latin America and the North Atlantic, the study follows a strategy and methodology similar to that employed in Lewis-Beck's 1988 cross-national study of the political business cycle model in the United States and Western Europe (see also Lewis-Beck 1986). Electoral effects are estimated on a country-by-country basis utilizing interrupted time-series analysis. The basic regression equation, which is estimated with ordinary least squares for four different indicators of economic policy performance, is as follows:

$$Y_t = b_o + b_1 X_{1t} + b_2 X_{2t} + b_3 X_{3t} + b_4 X_{4t} + b_5 X_{5t} + b_6 X_{6t} + e_t,$$

where $Y_t$ represents N quarterly time-series observations on the dependent economic performance variable; $X_{1t}$ is a dummy counter variable for time from 1 to N; $X_{2t}$ is a dummy variable for the quarter immediately preceding an election; $X_{3t}$ is a dummy variable for quarters in which elections occur; $X_{4t}$ is a dummy variable for the quarter immediately following an election; $X_{5t}$ is a dichotomous dummy variable scored 0 for observations before the first election in the time period and 1 for all observations after that election; $X_{6t}$ is a dummy counter variable of time scored 0 for observations before the first election and 1, 2, 3, . . . for observations after that election; and $e_t$ is the error term. In this model, $b_1$ estimates the time-series trend; $b_2$, $b_3$, and $b_4$ estimate the short-term shifts in economic performance associated with elections; $b_5$ estimates the postelectoral change in the level or intercept of the time series; and $b_6$ estimates the postelectoral change in trend or slope. For each additional election, two new dummies are incorporated into the model: one to measure the postelectoral slope, the other, the postelectoral shift in intercept. It may be noted that because the study is designed to replicate prior research in the Latin American context, rather than to develop appropriate macroeconomic models for individual countries, the basic statistical model is heavily biased against significance. The ordinary least squares estimates revolve around six or more dummy variables and counter variables. No economic variables are included, other than lagged values of the dependent variable that were added to several equations to address problems of autocorrelation.[1]

The specific indicators of performance that are used in the analysis were selected on the basis of quarterly availability, susceptibility to government manipulation, and presumed electoral salience. The indicators, which have been drawn from official sources as reported to the International Monetary Fund's *Monthly Financial Statistics* for 1981–92, are as follows: the quarterly rate of expansion of M1, the quarterly percentage change in the fiscal balance (controlling for inflation as measured by the consumer price index), the quarterly rate of exchange depreciation, and the quarterly rate of increase in consumer prices.

The first two indicators are at least partially responsive to government manipulation and therefore offer relatively direct evidence of policy priorities. To the extent that electoral considerations triumphed over prevailing economic wisdom and related pressures for austerity during the 1980s, the money supply and the government deficit might be expected to expand more rapidly than usual before elections and contract rapidly thereafter. To the extent that governments were not swayed by short-term electoral or political considerations, however, no cyclical relationships or electoral effects should emerge. Postelectoral outcomes should also evince no consistent pattern but vary, instead, with the policy commitments and economic constraints facing incoming administrations.

The behavior of the other two indicators might be expected to display just the opposite pattern in response to opportunistic policy choice. To the extent that leaders managed the economy in accordance with their electoral interests, they would have minimized short-run inflation and exchange depreciation in the preelectoral period. The two indicators are highly correlated longitudinally, as well as cross-sectionally; they have also proved to be politically sensitive. During the 1980s, the magnitude of the incumbent party's electoral loss in 21 Latin American presidential contests fluctuated directly with the rate of inflation and devaluation during the preelectoral quarter, with the inflation rate alone accounting for 61% of the variation across cases. The comparable figure for exchange depreciation is 55%.

The political significance of devaluation has long been recognized by scholars of dependent societies (Broad 1988; Cooper 1971, 472–513). Although valued as a major weapon in the orthodox arsenal for purposes of both stabilization and adjustment, the effects of devaluation are decidedly double-edged. In the long run, exchange rate depreciation may promote exports and otherwise bring an economy into alignment with international economic realities; in the short run, however, rapid shifts against nontradables undercut industrial growth, employment, price stability, and public-sector budgets (Sachs 1989, 17–23). The implications of devaluation are thus far-reaching and largely negative from the point of view of the voting public. It is precisely for this reason that governments may go to extremes to avoid devaluation, raising the future costs of stabilization and adjustment in the process. Presumably, however, the incentives and opportunities to avoid devaluation or other painful stabilization policies are not equal across a government's term of office. The probabilities of negotiating advantageous packages of international financial support that at least partially compensate for devaluation are likely to decline over the course of a presidential term, as are the probabilities of overcoming the initial political and economic costs of stabilization measures.

The sensitivity of Latin electorates to inflation is more puzzling from the point of view of existing literature. Indeed, indifference to inflationary pressures has been seen as a hallmark of vote-driven macroeconomic management. The political significance of inflationary pressures, however, is likely to differ at rates of 1,000%, rather than 5%. Since the outbreak of the debt crisis, consumer prices in Latin America have escalated at speeds exceeding anything known in recent U.S. or Western Europe experience. Real wages have suffered accordingly, leading voters in such countries as Argentina and Peru to rank inflation at the top of their list of national problems (*Caretas* 1987, 21; Catterberg 1989; Catterberg and Braun 1989, 366; Kuczynski 1990). The logic of the situation, however, does not necessarily push vote-maximizing politicians in the direction of fiscal and monetary austerity. To the extent that short-term office-seeking considerations triumph over prevailing orthodoxy, the preferred alternative is more likely to be wage and price controls, whose efficacy is immediate, albeit limited to the short run. Due to the immediate impact of devaluation on price levels, it may also be assumed that the political costs of inflation reinforce incentives to postpone devaluation to the postelectoral period.

Under the restrictive economic conditions of the 1980s, which limited the sustainability of attempts at combining exchange and price stability with monetary and fiscal expansionism, the predicted preelectoral effects are expected to be concentrated in the quarter immediately preceding elections. Nevertheless, I have explored the possibility of additional leads and lags and discuss these where appropriate. Given the constraints faced by Latin policymakers during the past decade, the tendencies toward postelectoral austerity are also expected to be more pronounced than those of preelectoral expansionism, with variations across time and space reflecting a combination of policy choice and perceived opportunities.

## STATISTICAL RESULTS

Tables 1 through 4 summarize the analysis of the impact of elections on economic performance. Table 1 explores the linkage between elections and expansionary government policies as measured by the rate of expansion of M1. Even though the regression coefficients achieve high levels of statistical significance in several instances, the estimations offer no support for traditional notions of a political business cycle. The direction of the effects of the preelectoral dummy variable is inconsistent across countries; the postelectoral dummy variable points in the direction of expansion, rather than contraction; and the slope and intercept variables suggest no common pattern. Simply in terms of the direction of the estimated effects, not one of the seven countries for which data are available fully conforms to the expected cyclical pattern. Moreover, of the regression coefficients that achieve statistical significance, one points in the direction of preelectoral expansionism, one suggests preelectoral austerity, three suggest postelectoral austerity, and five suggest postelectoral expansionism. Thus, the results are highly mixed, with the data pointing as strongly in the direction of countercyclical, as in the direction of cyclical, versions of the political business cycle.[2]

The estimations for change in the fiscal balance presented in Table 2 are also inconsistent with the predicted cyclical pattern. Evidence of electoral effects is limited; and again, the direction of such effects is heterogeneous across countries. Links can be drawn between elections and budgetary shifts only in the cases of Argentina and Venezuela, where the slope variables suggest opposite patterns involving postelectoral austerity in Venezuela and postelectoral expansionism in Argentina.[3]

The indicators of economic performance presented in the remaining two tables offer far stronger evidence of electoral effects. Looking first at the set of estimations presented in Table 3, the results indicate that elections significantly influenced the rate of inflation in seven of the eight countries. Again, however, the results are anything but supportive of the notion that Latin governments successfully manipulate the economy to maximize electoral gains. After controlling for autocorrelation, only Argentina and Peru begin to conform to the predicted pattern of declining rates of inflation during the preelectoral quarter followed by accelerating inflation in the postelectoral quarter. The preelectoral effects, however, are not statistically significant for either country. Moreover, in Argentina, inflation accelerated above the four-digit level during the electoral quarter itself, emphasizing the government's inability to manipulate the economy successfully. The evidence is also rather mixed for Peru. As indicated by the slope and intercept variables, the 1985 and 1990 elections had opposite impacts on inflation.

The estimates for Brazil and Bolivia are even less consistent with the notion that incumbents were

**TABLE 1**

**Ordinary Least Squares Estimates for Electoral Influences on the Quarterly Rate of Growth in M1: Latin America, 1982–1991**

| INDEPENDENT VARIABLES | ARGENTINA | BOLIVIA | ECUADOR | PERU | URUGUAY | VENEZUELA |
|---|---|---|---|---|---|---|
| Constant | 26.46 | −81.13** | 14.57** | −4.26 | 2.76 | 8.53 |
|  | (14.24) | (27.46) | (4.22) | (7.93) | (6.90) | (8.62) |
| Preelec. quarter | −30.45 | −4.00 | 10.67* | −29.70* | −3.00 | 12.83 |
|  | (33.00) | (34.30) | (3.85) | (11.56) | (17.74) | (8.12) |
| Elec. quarter | 257.30** | 34.50 | 3.80 | 37.70** | 9.81 | 8.13 |
|  | (33.29) | (37.39) | (3.53) | (10.86) | (17.47) | (8.07) |
| Postelec. quarter | 276.73** | −12.24 | 4.79 | 81.69** | −19.23 | 4.94 |
|  | (58.95) | (34.47) | (4.35) | (15.87) | (21.46) | (7.93) |
| Elec. 1 intercept | 35.17 | −153.58** | 8.87 | −37.24** | 8.46 | 9.15 |
|  | (66.65) | (44.61) | (4.65) | (10.11) | (17.56) | (8.07) |
| Elec. 1 slope | −7.11 | −7.06 | .030 | 3.30** | −1.40 | .00 |
|  | (9.87) | (5.86) | (.37) | (.89) | (3.09) | (.81) |
| Elec. 2 intercept | — | 2.97 | 8.77 | 1027.16** | — | −6.23 |
|  |  | (38.88) | (5.35) | (62.55) |  | (8.31) |
| Elec. 2 slope | — | −1.89 | .71 | −672.82** | — | 3.97** |
|  |  | (6.68) | (.78) | (55.16) |  | (1.09) |
| Time | .28 | 8.84** | −.18 | 1.00 | .55 | −.32 |
|  | (.57) | (2.17) | (.33) | (.59) | (.34) | (.78) |
| ΔM1(t − 1) | −.09 | .29 | −.71** | .53** | −.22 | −.52** |
|  | (.13) | (.15) | (.13) | (.11) | (.16) | (.14) |
| ΔM1(t − w) | −.29* | .05 | −.53** | — | −.20 | −.39* |
|  | (.13) | (.16) | (.15) |  | (.15) | (.14) |
| ΔM1(t − 3) | — | −.06 | −.69** | — | −.31* | −.66** |
|  |  | (.16) | (.13) |  | (.15) | (.15) |
| ΔM1(t − 4) | — | .19 | — | — | .43** | — |
|  |  | (.16) |  |  | (.16) |  |
| Box–Ljung–Q | 16.04 | 17.20 | 9.49 | 11.92 | 8.31 | 9.55 |
| F | 21.38** | 10.96** | 6.68** | 187.34** | 4.20** | 3.60** |
| Adj. $R^2$ | .80 | .74 | .62 | .98 | .43 | .39 |
| N | 43 | 44 | 39 | 43 | 44 | 45 |

*Note:* Entries are unstandardized regression coefficients with standard errors in parentheses. Box–Ljung–Q statistic is reported for number of autocorrelation function corresponding to .25N.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

maximizing their electoral chances. In Brazil, the rate of inflation accelerated before the presidential election of 1989, taking the rate of price increases for the previous 12 months above the 1,000% mark. Hyperinflation, signifying a loss of political control over the economy, also developed in Bolivia prior to the 1985 election. With an additional lead, the coefficient for the preelectoral quarter is positive and significant at the .0001 level; the sign for the electoral quarter, positive and statistically significant; and adjusted $R^2 = .728$. The data for Argentina, Bolivia, and Brazil thus suggest that approaching elections undercut economic performance; yet the results neither conform to the logic of the traditional business cycle literature nor buttress more recent claims regarding the policy proclivities of democratic governments in Latin America. Particularly in Brazil and Argentina, impending elections appear to have triggered serious macroeconomic disequilibria rather than a "stop-and-go" cycle calibrated to the political interests of incumbents. The Bolivian outcomes are even more anomalous in terms of the logic of the traditional business cycle literature. The preelectoral effects reflect a politically disastrous march to hyperinflation that forced President Hernan Siles Suazo to call elections in 1985, a year ahead of schedule (Blanco et al. 1985; Loza Telleria 1988; Sach and Morales 1988). The causal nexus presupposed by the political business cycle was thus actually reversed, making the electoral calendar a consequence, rather than a cause, of policy choice and performance. The same scenario was nearly repeated in Argentina, where the presidential transition was accelerated in 1989 to cope with deepening economic crisis.

**TABLE 2**

**Ordinary Least Squares Estimates for Electoral Influences on Quarterly Changes in the Fiscal Balance: Latin America, 1982–1991**

| INDEPENDENT VARIABLES | ARGENTINA | BOLIVIA | COLOMBIA | ECUADOR | PERU | URUGUAY | VENEZUELA |
|---|---|---|---|---|---|---|---|
| Constant | −54.54 | 201.80 | −217.18 | −187.65 | −15.18 | −106.12 | 144.60 |
| | (92.12) | (416.30) | (1,915.11) | (988.99) | (125.37) | (102.74) | (700.00) |
| Preelec. quarter | −75.81 | 458.66 | −279.35 | 140.45 | 143.40 | −36.81 | 450.53 |
| | (219.48) | (728.12) | (1,599.75) | (760.50) | (197.79) | (314.66) | (939.75) |
| Elec. quarter | −24.47 | 716.49 | −443.15 | 23.51 | 25.14 | −8.12 | 458.42 |
| | (220.80) | (738.25) | (1,667.15) | (652.03) | (190.21) | (315.90) | (960.14) |
| Postelec. quarter | −487.49 | 392.27 | 1,934.08 | 148.72 | 206.74 | 96.20 | 4,818.99** |
| | (271.10) | (952.61) | (1,659.55) | (537.83) | (222.25) | (391.13) | (959.97) |
| Elec. 1 intercept | 450.61* | −1,819.46* | −3,998.13 | 714.08 | 100.49 | −124.07 | 1,306.91 |
| | (216.69) | (680.25) | (2,237.61) | (1,127.66) | (171.11) | (320.91) | (934.95) |
| Elec. 1 slope | −106.72** | 155.25 | 258.57 | —[a] | 1.51 | 52.26 | −61.93 |
| | (36.25) | (82.87) | (296.91) | | (13.26) | (56.63) | (79.76) |
| Elec. 2 intercept | — | — | −1,180.90 | −272.64 | 500.94 | — | −1,329.62 |
| | | | (1,999.98) | (655.97) | (310.06) | | (1,039.98) |
| Elec. 2 slope | — | — | −263.62 | 130.00 | −83.66 | — | 345.69** |
| | | | (178.96) | (64.81) | (68.20) | | (126.33) |
| Elec. 3 intercept | — | — | −1,180.90 | — | — | — | — |
| | | | (1,999.98) | | | | |
| Elec. 3 slope | — | — | 37.17 | — | — | — | — |
| | | | (605.91) | | | | |
| Time | .17 | −42.62 | 37.71 | −19.49 | −5.70 | 2.95 | −23.57 |
| | (3.93) | (29.58) | (287.72) | (51.39) | (9.54) | (4.42) | (70.55) |
| Δ fiscal bal. (t − 1) | −.65** | .69** | — | — | — | — | — |
| | (.16) | (.19) | | | | | |
| Δ fiscal bal. (t − 2) | −.19 | −.63** | — | — | — | — | — |
| | (.17) | (.19) | | | | | |
| Δ fiscal bal. (t − 3) | −.56** | −.58** | — | — | — | — | — |
| | (.18) | (.18) | | | | | |
| Δ fiscal bal. (t − 4) | −.32 | — | — | — | — | — | — |
| | (.18) | | | | | | |
| Box–Ljung–Q | 7.87 | 7.75 | 8.42 | 4.42 | 8.54 | 14.65 | 10.07 |
| F | 4.14** | 3.95** | .85 | 1.54 | 1.00 | .74 | 4.69** |
| Adj. $R^2$ | .43 | .49 | −.03 | .11 | .00 | −.03 | .39 |
| N | 43 | 29 | 48 | 31 | 47 | 48 | 48 |

*Note*: Entries are unstandardized regression coefficients with standard errors in parentheses. Box–Ljung–Q statistic is reported for number of auto correlation function corresponding to .25N.

[a] For Ecuador, the first slope variable was perfectly collinear with the counter for time and was therefore dropped from the analysis.

*$p \leq .05$, two-tailed test.

**$p \leq .01$, two-tailed test.

With respect to postelectoral trends, the pattern is mixed. As suggested by the slope variable, price stabilization followed six elections: Argentina 1989, Bolivia 1985, Ecuador 1988, Peru 1990, Uruguay 1989, and Venezuela 1988. On the other hand, the coefficients for the slope variables for the Peruvian election of 1985 and the Venezuelan election of 1983 suggest a postelectoral pattern of accelerating inflation. If the data are reestimated with additional quarterly leads and lags to allow for the possibility of earlier preelectoral manipulation or delayed postelectoral effects, the results are virtually identical.[4]

The estimations presented in Table 4 show a similar pattern of electoral effects. In five of the eight countries, the coefficients for the electoral variables indicate that the occurrence of an election significantly accelerated the rate of exchange depreciation. As estimated by adjusted R-squared, electoral effects account for over 95% of quarterly variation in the Argentine rate of exchange depreciation. The comparable figure for Venezuela, where two of three major devaluations in the 1980s followed presidential elections, is 64%. The electoral dummy variables also capture significant amounts of variation in the quar-

TABLE 3

**Ordinary Least Squares Estimates for Electoral Influences on Inflation: Latin America, 1982–91**

| INDEPENDENT VARIABLES | ARGENTINA | BOLIVIA | BRAZIL | COLOMBIA | ECUADOR | PERU | URUGUAY | VENEZUELA |
|---|---|---|---|---|---|---|---|---|
| Constant | 27.78<br>(15.26) | −77.83*<br>(30.00) | 3.70<br>(6.04) | 8.83**<br>(2.52) | 1.47<br>(2.04) | 11.56<br>(34.51) | 7.06**<br>(1.36) | 8.13*<br>(3.46) |
| Preelec. quarter | −17.36<br>(45.46) | −10.28<br>(50.62) | 67.84**<br>(17.68) | .35<br>(1.28) | −2.42<br>(2.93) | −13.89<br>(44.62) | 1.29<br>(4.17) | 2.51<br>(3.34) |
| Elec. quarter | 186.42**<br>(45.66) | 79.46<br>(51.73) | 100.37**<br>(18.13) | −.77<br>(1.33) | −.61<br>(2.47) | 43.79<br>(42.70) | 1.32<br>(4.19) | 3.22<br>(3.44) |
| Postelec. quarter | 385.72**<br>(53.19) | 13.45<br>(53.62) | 309.05**<br>(22.56) | −1.36<br>(1.27) | −.15<br>(2.95) | 177.17**<br>(53.97) | −4.19<br>(5.19) | −3.10<br>(3.46) |
| Elec. 1 intercept | 155.39**<br>(39.37) | −171.37**<br>(51.07) | −44.57<br>(24.76) | −2.19<br>(1.84) | −6.44<br>(3.27) | −96.03*<br>(39.73) | 10.36*<br>(4.25) | 2.80<br>(3.45) |
| Elec. 1 slope | −22.62**<br>(5.60) | −16.00**<br>(4.64) | 5.48<br>(4.34) | −.09<br>(.30) | 8.85<br>(.27) | 12.73**<br>(3.30) | −1.69*<br>(.75) | .79*<br>(.34) |
| Elec. 2 intercept | — | 22.93<br>(63.51) | — | .45<br>(1.49) | 5.45<br>(3.26) | 437.11**<br>(91.89) |  | 8.11**<br>(4.74) |
| Elect. 2 slope | — | 4.57<br>(8.28) | — | .17<br>(.13) | −1.06**<br>(.33) | −137.13**<br>(20.80) |  | −2.37**<br>(.57) |
| Elec. 3 intercept | — | — | — | 1.53<br>(2.09) | — | — |  | — |
| Elec. 3 slope | — | — | — | −.60<br>(.44) | — | — |  | — |
| Δ CPI (t − 1) | — | — | .27**<br>(.07) | .08<br>(.13) | — | −.33**<br>(.11) |  | .05<br>(.16) |
| Δ CPI (t − 2) | — | — | — | −.66**<br>(.14) | — | −.27**<br>(1.00) |  | −.33*<br>(.15) |
| Δ CPI (t − 3) | — | — | — | — | — | — |  | — |
| Time | .53<br>(.69) | 11.96**<br>(2.16) | 1.06**<br>(.28) | .09<br>(.30) | .46*<br>(.19) | 1.47<br>(2.50) | .23**<br>(.06) | −.49<br>(.31) |
| Box–Ljung–Q | 6.34 | 9.96 | 11.36 | 15.78 | 14.09 | 12.75 | 16.55 | 7.95 |
| F | 30.38** | 7.46** | 74.55** | 3.52* | 5.04** | 14.80** | 7.87** | 5.69** |
| Adjusted $R^2$ | .79 | .52 | .92 | .40 | .41 | .75 | .47 | .51 |
| N | 48 | 48 | 47 | 46 | 48 | 46 | 48 | 46 |

*Note*: Entries are unstandardized regression coefficients with standard errors in parentheses. Box–Ljung–Q statistic is reported for number of autocorrelation function corresponding to .25N.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

terly rate of exchange depreciation for Bolivia, Brazil, and Ecuador. If adjustments are introduced to cope with a severe outlier problem, a sixth country, Peru, which experienced severe exchange rate depreciation during the election of 1990, may be added to this list.[5] In all six of these countries, exchange depreciation accelerated during the electoral or postelectoral quarter, supporting the idea that governments postponed devaluation for political purposes. In no instance, however, do the results conform to a cyclical model of macroeconomic management involving a significant slowing of depreciation in the preelectoral period. The direction of the effects for the preelectoral quarter is mixed, and the only coefficients for the preelectoral or electoral quarter that are statistically significant run diametrically counter to such an expectation. Again, allowing for additional leads and lags does not appreciably alter the statistical results, nor do adjustments designed to correct for possible problems of multicollinearity in those cases in which the number of observations before and after the electoral event are seriously uneven.[6]

Shifting the analysis to interim congressional elections, which are held in several countries, alters the results presented mainly by weakening the evidence suggesting electoral effects. For Ecuador, electoral effects largely disappear, with the exception of the pattern of postelectoral devaluation identified above. The sole reversal in the direction of effects is for Table 1, where a positive and statistically significant slope variable emerges for the 1986 congressional election, suggesting a pattern of postelectoral, rather than preelectoral, expansionism. For Colombia, electoral effects also remain limited. For Argentina and Brazil,

TABLE 4

**Ordinary Least Squares Estimates for Electoral Influences on the Quarterly Rate of Exchange Depreciation: Latin America, 1982–1991**

| INDEPENDENT VARIABLES | ARGENTINA | BOLIVIA | BRAZIL | COLOMBIA | ECUADOR | PERU | URUGUAY | VENEZUELA |
|---|---|---|---|---|---|---|---|---|
| Constant | 40.99**<br>(14.42) | −129.16<br>(65.44) | 7.20<br>(8.29) | 1.75<br>(1.52) | −12.52<br>(7.65) | −.06<br>(47.25) | 12.02<br>(8.02) | −1.91<br>(9.79) |
| Preelec. quarter | −22.43<br>(42.96) | −57.88<br>(110.40) | 90.86**<br>(25.40) | −1.44<br>(.77) | −5.83<br>(8.43) | −16.55<br>(74.54) | 2.48<br>(24.56) | −7.23<br>(13.14) |
| Elec. quarter | 1,510.67**<br>(43.15) | 582.44**<br>(112.83) | 138.73**<br>(25.50) | −.76<br>(.84) | −13.41<br>(7.19) | 47.79<br>(71.68) | 1.50<br>(24.66) | −7.83<br>(13.43) |
| Postelec. quarter | 7.77<br>(50.27) | −12.54<br>(116.95) | 241.24**<br>(31.57) | .02<br>(.79) | 22.32**<br>(8.51) | 29.36<br>(83.76) | .74<br>(30.53) | 107.95**<br>(13.43) |
| Elec. 1 intercept | 119.66**<br>(37.20) | −211.70<br>(111.39) | −34.14<br>(25.90) | −.82<br>(1.16) | −27.85**<br>(9.90) | −60.00<br>(64.49) | 6.74<br>(25.05) | −9.03<br>(13.08) |
| Elec. 1 slope | −18.86**<br>(5.29) | −33.22**<br>(10.11) | 5.91<br>(4.57) | .09<br>(.20) | −.20<br>(.81) | 7.75<br>(5.00) | −1.46<br>(4.42) | .62<br>(1.12) |
| Elec. 2 intercept | — | 144.59<br>(138.51) | — | −4.60**<br>(1.33) | −.79<br>(9.41) | −154.31<br>(116.85) | — | 7.39<br>(14.55) |
| Elec. 2 slope | — | 13.99<br>(18.05) | — | −.18<br>(.11) | −3.56**<br>(1.03) | −7.90<br>(25.70) | — | −2.84<br>(1.77) |
| Elec. 3 intercept | — | — | — | −.46<br>(1.41) | — | — | — | — |
| Elec. 3 slope | — | — | — | −.35<br>(.30) | — | — | — | — |
| Δ exchange (t − 1) | — | — | — | .69**<br>(.15) | −.34*<br>(.14) | — | — | — |
| Δ exchange (t − 2) | — | — | — | −.54**<br>(.19) | −.33*<br>(.14) | — | — | — |
| Time | 6.31<br>(.65) | 18.10**<br>(4.71) | 1.30**<br>(.36) | .23<br>(.20) | 2.15**<br>(.71) | 1.61<br>(3.60) | .07<br>(.35) | .30<br>(.99) |
| Box–Ljung–Q | 7.40 | 12.02 | 11.25 | 12.74 | 14.87 | 5.75 | 4.60 | 14.45 |
| F | 227.67** | 8.69** | 29.49** | 9.05** | 3.13** | 1.43 | .05 | 11.61** |
| Adj. $R^2$ | .97 | .57 | .78 | .68 | .32 | .07 | −.14 | .64 |
| N | 48 | 48 | 48 | 46 | 46 | 48 | 48 | 48 |

*Note:* Entries are unstandardized regression coefficients with standard errors in parentheses. Box–Ljung–Q statistic is reported for number of autocorrelation function corresponding to .25 N.
*$p \leq .05$, two-tailed test.
**$p \leq .01$, two-tailed test.

focusing the analysis on congressional elections has a greater impact. Although the electoral effects are weaker than those reported above, the statistical results for both countries become more consistent with traditional versions of the political business cycle. For Argentina, the coefficients for exchange depreciation in the preelectoral and electoral quarters become negative and statistically significant; and the statistically significant coefficient for the rate of change in M1 during the electoral quarter disappears. For Brazil, the coefficients measuring inflation and exchange depreciation remain positive and statistically significant for the postelectoral quarter. However, the countercyclical preelectoral and electoral effects for inflation disappear; and the coefficient for exchange depreciation in the electoral quarter becomes negative and significant. These results reflect the impact of heterodox adjustment programs—notably, the "Plan Austral," which was adopted immediately before the 1985 Argentina congressional elections, and the "Cruzado Plan," implemented just prior to the Brazilian congressional elections of 1986. By combining a wage and price freeze with a fixed exchange rate, both plans achieved short-term success in controlling inflationary pressures; however, efforts to replicate these successes at later stages of the same administrations ended in disaster (see Pang 1989; Smith 1990). Thus, for both Brazil and Argentina, there are clear signs that newly elected governments managed to manipulate the economy for electoral gain and that these efforts had destabilizing consequences.

## ECONOMIC PERFORMANCE AND POLICY CHOICE

The preceding analysis indicates that macroeconomic performance has fluctuated with the electoral calendar in South America, but that the impact of elections has varied significantly across time and space. Far from conforming to a simple model of preelectoral expansionism and postelectoral contraction, the statistical results suggest three different patterns: the null, the cyclical, and the countercyclical. The variations among them reflect the importance of institutional arrangements and policy choice.

The importance of the first of these factors is suggested by the pattern of null results, corresponding most closely in country terms to the findings for Uruguay and Colombia. In both countries, relatively independent bureaucracies have guided macroeconomic policy in response to market signals, minimizing electoral influences on performance, particularly in the area of exchange policy. Throughout the 1980s the Uruguayan peso floated independently; in Colombia, the currency was adjusted almost daily in accordance with price shifts at home and abroad. As a result, exchange rate policy was relatively depoliticized, limiting tendencies to postpone devaluation until after elections. Underlying these arrangements was the development of a relatively strong elite consensus on economic management (Anglade and Fortín 1990, 291–92; Urrutia 1990).

The importance of the institutional setting is further emphasized by the Bolivian experience. The presidential election of 1985, which was followed by a major devaluation, took place under a fixed-exchange-rate regime that produced a highly overvalued currency. The pattern of policy formation surrounding the 1989 election was very different. After 1987, the exchange rate was adjusted by the central bank on the basis of a daily currency auction, which reduced the spread between the official rate and the black market rate to less than 1% (Cariaga 1990). Hence, if the post-1987 period is deleted from the analysis, the adjusted R-squared for the model estimated in Table 4 increases from .567 to .843. As for Uruguay and Colombia, these results reflect the impact of efforts at elite consensus formation, which paved the way for the depoliticization of macroeconomic policy (Foro Económico 1987; Gamarra 1989; Sachs and Morales 1988). By the end of the decade, other countries, including Chile, had embarked on a similar course, adjusting key economic policy instruments in response to market signals, enhancing the independence of central banks, augmenting the role of technocrats, and otherwise insulating the day-to-day management of the economy from political pressures.

The remaining two patterns of results indicate that where economic policy was not depoliticized, elections shaped macroeconomic performance, albeit in contradictory ways. One set of electorally related results approximates the hypothesized business cycle in the sense that economic performance improved prior to or during elections and/or declined in their wake. Truly cyclical effects, however, are evident only for interim elections in Argentina and Brazil, where the rate of inflation and exchange depreciation dropped going into elections in response to the application of unorthodox policies and accelerated in their wake. For presidential elections, the effects were consistently asymmetrical and concentrated in the postelectoral period. In Peru (1985) and Venezuela (1983), incumbents warded off economic breakdown in the preelectoral period, albeit without significantly diverting the economy from its existing course and without assuring the reelection of incumbent forces. As in the case of the Argentine and Brazilian interim elections, performance deteriorated subsequent to elections, largely because the newly elected governments in both of these countries underestimated the magnitude of the adjustment that was required by international conditions. The common pattern is reflected in the positive and statistically significant slope variables for inflation.

Far more prevalent was the electorally counterproductive pattern of results—the "anti-political business cycle." In Argentina (1989), Bolivia (1985), Brazil (1989), Ecuador (1984), Ecuador (1988), Peru (1990), and Venezuela (1988), inflation, exchange depreciation, and other symptoms of economic disequilibrium mounted before elections and/or were stabilized in their wake. Again electoral effects are notable chiefly in terms of postelectoral indicators of exchange depreciation and inflation, although for Argentina, Bolivia, and Brazil, the dummy variables capture a significant pattern of preelectoral disintegration.

What explains the differences between these two electorally related patterns? To what extent can they be linked with the policy choices of decision makers, rather than factors beyond their control? Given the nature of the statistical findings, especially the surprising strength of the electoral effects across different countries and elections, it would be difficult, if not perverse, to dismiss the existence of a relationship between elections and economic performance. Electoral effects may be underestimated by the exclusion of relevant variables, but specification errors arising from the common influence of additional variables on the independent and dependent variables are implausible. A sudden shift in the terms of trade, for example, might explain postelectoral devaluation in a single case; but it is difficult to conjure up a scenario in which a number of elections held at varying points in time in six different countries accidentally coincided with other causally relevant events so as to produce a series of postelectoral devaluations. Likewise, the lack of uniformity in postelectoral performance patterns suggests that the effects cannot be simply ascribed to the uncertainties of economic actors. Postelectoral performance was characterized by increased instability as well as economic stabilization.

The differences between the two electorally related patterns reflect divergent sets of policy choices. The pattern involving postelectoral economic decline can be linked with relatively unorthodox sets of policy choices, including wage and price controls, fixed exchange rates, and other notable deviations from market mechanisms. In contrast, the pattern involving postelectoral stabilization may be linked with relatively orthodox programs of stabilization and adjustment. Given that economic instability was far easier to achieve than stability under the economic conditions of the 1980s, the latter linkage is particularly notable. The statistically significant postelectoral effects registered in Argentina (1989), Bolivia (1985), Brazil (1989), Ecuador (1988), Peru (1990), Venezuela (1988), and Uruguay thus provide strong indications of the importance of elections.

Contemporary assessments of policy choice and performance in these latter cases buttress this line of argument. In each instance, new administrations risked short-term political losses to achieve economic gains, and adopted comparatively orthodox—and, in some instances, drastic—policy remedies. Thus, the economic program that followed the Venezuelan election of 1988 has been described as "one of the most aggressive adjustment programs in Latin America" (Larraín 1990, 247). The economic plan announced by Fernando Collar de Melo in early 1990 in the wake of the 1989 Brazilian election was likewise hailed by the Inter-American Development Bank as "the most ambitious stabilization and adjustment program ever attempted in Brazil's history" (1990, 71; hereafter IADB). In a similar vein, the Bolivian New Economic Plan of 1985 has been acclaimed not only as "the most dramatic stabilization effort in Latin American history" but as "the most radical liberalization since Ludwig Erhard's 1948 reforms that launched the German economic miracle" (Harberger, 1988, 7; Williamson 1990, 357). Within three weeks of assuming office, the newly elected president, Victor Paz Estenssoro, had announced a sweeping program of liberalization and macroeconomic stabilization that reduced the annual rate of inflation from 11,748% in 1985 to 276% in 1986 and the budget deficit as a percentage of gross domestic product from 10.9% to 1.7% (Mann 1989, 375–397; IADB 1990, 60). Significant shifts in the direction of orthodoxy likewise followed the election of new authorities in Ecuador in 1988 (IADB 1989, 327); Peru in 1990 (IADB 1991, 147–49); and Uruguay in 1989. Even in Argentina, where the victorious candidate in the 1989 election had campaigned on a populist, antiorthodox program, postelectoral stabilization reflected the introduction of a revolutionary program of orthodox fiscal and monetary reform (CEPAL 1989, 16–17).

In short, the postelectoral trend toward stabilization can be traced back to newly elected administrations who eschewed the temptations of expansionism in favor of policies emphasizing austerity, stabilization, and relatively orthodox solutions to problems of macroeconomic instability. Shifts in the opposite direction, on the other hand, reflected the choices of governments that avoided policy orthodoxy. The predominance of the former trend suggests that competitive elections should perhaps be seen less as threats to economic stability than as catalysts for policy reform and responsible macroeconomic management. During the 1980s, stabilization and adjustment represented *the* key economic challenges confronting Latin governments. Orthodox policy choices offered no guarantee of economic success but unorthodox policies emphasizing growth at the expense of stabilization and adjustment (as in post-1985 Peru) were a certain recipe for disaster.

The variations in preelectoral patterns of performance may likewise be linked with policy choice. In the four instances in which a statistically significant and electorally counterproductive pattern of economic deterioration was noted in the preelectoral or electoral quarter (Argentina 1989, Bolivia 1985, Brazil 1989, and Peru 1990), outgoing governments had pursued unorthodox policies, exhausted the expansionary possibilities of the economy, and all but eliminated their freedom to maneuver prior to the approach of elections. As a result, preelectoral performance was less the product of policy innovation than of inertia in the face of mounting inflation, capital flight, and a general loss of business confidence. Although political uncertainties played a role, the economic breakdown in all four instances was arguably driven less by the approach of elections than by their recession into the distant past. Governmental capacity to innovate internally and bargain externally diminished through time as economic performance deteriorated, political support declined, and policy options narrowed.

As indicated by the repeated pattern of postelectoral stabilization, the failure of authorities to undertake needed adjustments in the preelectoral period was not peculiar to Bolivia (1985), Brazil (1989), Argentina (1989) and Peru (1990). Prior to the 1983 Venezuelan election, for example, the incumbents warded off devaluation through a combination of price controls, import curbs, and a moratorium on debt payments (Economist Intelligence Unit 1983). Devaluation and other adjustment measures were similarly postponed until after the December 1988 election (Economist Intelligence Unit 1988). By the time new authorities were to assume office, the parallel market premium approached 200%, liquid international exchange reserves had plummeted to a level insufficient to cover 15 days of imports, and the fiscal deficit exceeded 10% of gross domestic product (Larraín 1990, 248). Hence as in Argentina, Bolivia, Brazil, and Peru, the newly elected administration was placed under pressure to turn to the International Monetary Fund and adopt a drastic and politically costly program of austerity and adjustment. Other elections were similarly associated with short-term measures designed to bolster economic performance, such as price controls, currency overvaluation, and the suspension of debt payments. But while authorities may have attempted to manipulate economic performance for electoral gain, the statistical results indicate that the impact of policy choice on performance ranged from the ineffectual to the counterproductive. The latter outcome was characteristically a product of long-term policy choice and the related exhaustion of alternatives to severe orthodoxy or accelerating disequilibrium.

Party control, ideology, electoral base, and prospective policy commitments, whose importance has been highlighted in work on the North Atlantic region (e.g., Hibbs 1977, 1987; Hibbs and Fassbender 1981), played a limited role in policy choice. The same party won the Venezuelan elections of 1983 and 1988, for example; but as reflected in the slope variables for inflation, the two new administrations adopted very different policies. During the 1980s, conservative governments enjoying the support of the business community pursued relatively heterodox policies (e.g., post-1985 Brazil, post-1984 Ecuador); democratic socialists opted for orthodoxy (post-1988 Ecuador); and populists chose orthodoxy (post-1989 Argentina, post-1988 Venezuela), as well as heterodox expansionism (post-1985 Peru). Economic conditions establish some basis for explaining these variations, but policy choice cannot be reduced to a simple question of economic constraint. As emphasized by the recent history of Argentina, Bolivia, Brazil, and Peru, four-digit, and even five-digit, inflation does not necessarily lead to the adoption of stabilization programs; nor does the exhaustion of available exchange reserves. Similarly, austerity has been maintained in the face of relative price and exchange stability and expansionism under precisely the opposite set of conditions.

More important than economic conditions per se are policymakers perceptions of economic options and constraints, which have varied significantly over time and space and shaped policy choice. Of defining importance for new administrations during the 1980s were the perceived successes and failures of their predecessors. Accordingly, the failures of unorthodox or heterodox approaches in post-1983 Argentina, post-1982 Bolivia, post-1985 Brazil, post-1984 Ecuador, post-1985 Peru, and post-1983 Venezuela paved the way for major doses of austerity and orthodoxy after the next election. Similarly, governments that attempted to stabilize price levels and augment international reserves at the expense of economic growth, as in post-1979 Ecuador and post-1980 Peru, created incentives and opportunities for their successors to pursue a less orthodox path. From this perspective, the two electorally driven patterns of economic performance are not truly competitive but, rather, serially linked in a broader cycle involving the alternation of relatively orthodox and unorthodox policy formulas. As suggested in the traditional literature, elections mark the points of policy alternation; however, the full cycle has extended over a period of more than one national administration and has been broken only where efforts at elite consensus formation have at least partially depoliticized the economic policy process.

Comparative analysis of the link between policy choice and economic outcomes thus buttresses the statistical findings. During the 1980s, elections in South America influenced policy and performance—but less because established governments successfully manipulated the economy to achieve electoral gains than because newly elected leaders adopted economic programs designed to address serious macroeconomic problems. The impact of these programs varied with policy choice, which ranged from the highly orthodox (e.g., post-1988 Venezuela) to the heterodox (post-1985 Peru). Given international economic constraints, less orthodox sets of policy choices were associated with mounting postelectoral economic disequilibrium and more orthodox ones with stabilization. The approach of elections arguably delayed the adoption of appropriate macroeconomic policies, but significant preelectoral shifts in economic performance were registered mainly where the previous pattern of policy choice had reduced government options to the point of producing electorally counterproductive sets of economic outcomes. Under such circumstances, elections represented a solution rather than a threat to macroeconomic stability. The *approach* of elections threatened effective macroeconomic management far less than did their *recession* into the distant past.

## CONCLUSION

The Latin American experience over the past decade suggests that economic performance fluctuates with the electoral calendar—but less because governments successfully manipulate the economy to garner votes than because elections pave the way for the adoption of new policy approaches. At worst, elections have delayed the implementation of appropriate macroeconomic policies, without significantly altering the trajectory of the economy; more characteristically, elections have facilitated the implementation of policies designed to address serious problems of macroeconomic instability. Thus, to the extent that an electoral–economic cycle can be identified in Latin America, it revolves around postelectoral, rather than preelectoral, policy choice, much as in the United States. As assessed by indicators of government deficits, changes in the money supply, inflation, and exchange depreciation, not a single country conforms to the traditional political business cycle idea of enhanced preelectoral economic performance. The more evident trend has been diminished preelectoral performance, particularly where the previous pattern of policy choice has clashed with the prevailing international orthodoxy.

Postelectoral economic outcomes have varied through time and space, principally in response to institutional arrangements and policy choice. In Uruguay, Colombia, and post-1985 Bolivia, the electoral calendar has had a limited impact on the economy, suggesting the development of a relatively technocratic, or nonelectoral, model of policy choice and performance that is rooted in elite consensus and the depoliticization of key policy decisions. Elsewhere, postelectoral outcomes have varied with perceptions of economic opportunity and related patterns of policy choice. In post-1985 Peru and post-1983 Venezuela, where politicians underestimated the magnitude of the adjustment required by changing international conditions, economic stability deteriorated in the wake of the elections in accordance with the predictions of the traditional political business cycle literature. More commonly, however, performance improved with elections, largely because new administrations braked a process of economic disintegration. By the time elections approached, Latin governments in the 1980s were often less the master than the victim of the economy. Under conditions of severe crisis, economics even drove the political calendar, rather than the reverse.

Three central conclusions are suggested by these findings. First, the analysis highlights the problematic disjunction between policy choices and outcomes. Politicians may wish to manipulate the economy for electoral gains; but their capacity to do so is distinctly limited, particularly in an environment of economic instability and constrained policy options. As emphasized by the experience of pre-1985 Bolivia, even the money supply and fiscal deficit may not be controlled by the national administration, much less such outcomes as inflation or economic growth. Although literature on the United States and Western Europe has not ignored the existence of this problem, its potential significance for understanding policy outcomes in other settings is far greater.

Second, the findings indicate that the impact of elections varies with the policymaking infrastructure, undercutting generalizations about regime effects. The ways in which policy is made, the kinds of expertise available to policymakers, and the policy instruments under the control of elected authorities differ from democracy to democracy, even within Latin America, creating widely varying incentives and opportunities for political manipulation of the economy. Specific hypotheses regarding the relationship between economics and democratic politics are consequently problematic, as are broad characterizations of Latin American democracies.

Third, to the extent that elections shape macroeconomic policy choice and performance, they may enhance rather than undermine the ability of governments to respond appropriately to macroeconomic challenges. This possibility has been neglected by the traditional business cycle literature, which has portrayed elections as disruptive events that interfere with otherwise rational patterns of macroeconomic management. The relatively stable and consensual environment surrounding policy formation in the advanced industrial democracies may be partially responsible. As attention shifts to more unstable and conflictual settings, elections take on a different significance. Periodic electoral contests provide leaders with opportunities to mobilize the political support necessary to address intractable economic problems with limited resources. Thus, instead of encouraging politicians to sacrifice the performance of the economy in order to gain votes, elections may create conditions that permit leaders to set aside immediate political concerns in favor of the pursuit of prevailing economic wisdom.

These conclusions suggest that the nexus between elections and economic performance is captured more effectively by what might be called a "political capital model" than by its more conventional theoretical alternative, the "political business cycle." In emphasizing incentives at the expense of opportunities, the latter model neglects the slippage between policy choice and outcomes, the importance of institutional and other constraints, and the shifts in the relative power of state and society that may be expected to occur over the course of an electoral cycle (Gaubatz 1991). The more remote the last election, the more limited the political capital of government leaders and the greater the restrictions on state initiatives. For a variety of reasons (e.g., the electoral salience of economic performance and the importance of international bargaining position), these restrictions are likely to be severe in peripheral societies. However, the political capital model captures a broader reality having to do with the relative capacity of state actors to introduce and implement policies. Precisely because electoral victory strengthens the hand of government authorities, elections are more appropriately portrayed as catalysts for innovation and effective policy performance than as obstacles to sound macroeconomic management.

## Notes

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1. These problems varied significantly across cases and indicators of economic performance, requiring in some instances third- and even fourth-order corrections. Autocorrelation was assessed on the basis of the Box–Ljung or Q-statistic, as well as by regressing the residuals of the dependent variable on all independent variables plus the first-to-fourth-order lagged residuals of the dependent variable.

2. These results remain quite stable when calculated with additional electoral leads and lags, with the exception of Ecuador. In this case, leading the preelectoral dummy variable raises the coefficients for both intercept variables to statistically significant levels.

3. It may be noted, however, that the results for presented in Table 2 are partially misleading with respect to Bolivia, due to severe outlier problems. If the data are adjusted to deal with these problems, the coefficient for the postelectoral quarter becomes negative and statistically significant. It may also be noted that additional leads and lags alter the results for Colombia. The fiscal situation deteriorated sharply two quarters after the 1982 election, making the postelectoral dummy variable negative and statistically significant at the t + 1 observation.

4. With the exception of Bolivia that I have noted, taking earlier observations fails to enhance the significance of the data for the preelectoral quarter. However, some improvements are registered when later values are considered (most notably for Venezuela), as is indicated by the following estimation:

$$\mathrm{CPI} = \begin{array}{ccccccc} 6.738 & + & 1.911\ \mathrm{PRE} & + & 2.113\ \mathrm{ELECTORAL} & + & 1.738\ \mathrm{POST} \\ (2.582) & & (2.479) & & (2.564) & & (2.720) \end{array}$$

$$\begin{array}{llll} +\ 13.937\ \text{POST}_{t+1} & -\ 1.355\ E_1\ \text{INTERCEPT} & +\ .918\ E_1\ \text{SLOPE} \\ \quad (2.563) & \quad (2.668) & \quad (.253) \end{array}$$

$$\begin{array}{ll} +\ 8.348\ E_2\ \text{INTERCEPT} & -\ 1.683\ E_2\ \text{SLOPE} \\ \quad (3.949) & \quad (.441) \end{array}$$

$$\begin{array}{ll} -\ .216\ \text{CPI}_{t-1} & -\ .386\ \text{TIME}, \\ \quad (.115) & \quad (.231) \end{array}$$

where the numbers in parentheses are standard errors, F = 12.078, and adjusted $R^2 = .730$. It may also be noted that the results are not significantly distorted by the asymmetry in the number of observations before and after the electoral event in cases in which elections were held at the end of the decade. To assess this possibility, the electoral intercept and slope variables were dropped successively from the models for Argentina, Bolivia, Brazil, Colombia, Peru, and Uruguay. No significant changes are registered for Brazil, Bolivia, and Colombia. For Argentina and Uruguay, the coefficients for the slope and intercept variables cease to be significant—but without changing the direction or magnitude of other effects or significantly reducing adjusted R-squared. For Peru, the coefficient for the second intercept variables ceases to be significant; but the results remain otherwise similar.

5. In the Peruvian case, the "shock" program introduced in September 1988 masks the electoral effects. With this quarter excluded from the analysis, the ordinary least squares estimates for Peru are

$$\text{Exchange depreciation} = \begin{array}{ll} 3.545 & +\ 18.895\ \text{PRE} \\ (13.872) & (20.102) \end{array}$$

$$\begin{array}{lll} +\ 80.941\ \text{ELECTORAL} & +\ 30.531\ \text{POST} & -\ 57.206\ \text{1ST INTERCEPT} \\ \quad (19.105) & \quad (22.587) & \quad (17.651) \end{array}$$

$$\begin{array}{lll} +\ 5.054\ E_1\ \text{SLOPE} & -\ 93.961\ E_2\ \text{INTERCEPT} & -\ 2.802\ E_2\ \text{SLOPE} \\ \quad (1.418) & \quad (31.334) & \quad (6.911) \end{array}$$

$$\begin{array}{ll} +\ .062\ \text{EXCHANGE DEPRECIATION}_{(t-1)} & +\ 1.085\ \text{TIME}, \\ \quad (.043) & \quad (1.034) \end{array}$$

with the F-ratio 9.718 and adjusted $R^2 = .636$. The coefficients for the electoral quarter, both intercept variables, and first slope variable are statistically significant at the .01 level. With a dummy variable included for the third quarter of 1988, the F-ratio increases to 67.626, and adjusted $R^2$ to .935; but the electoral effects appear virtually identical.

6. The key exception is Colombia, where the coefficient for the lagged postelectoral dummy is positive and statistically significant. With the additional dummy, the coefficient of the preelectoral dummy also becomes statistically significant, thereby bringing the statistical results partially into line with traditional versions of the political business cycle.

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# POWER POLITICS AND INTERNATIONAL TRADE

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*Recent literature attributes the relative scarcity of open international markets to the prisoner's dilemma structure of state preferences with respect to trade. We argue that the prisoner's dilemma representation does not reflect the most critical aspect of free trade agreements in an anarchic international system, namely, their security externalities. We consider these external effects explicitly. Doing so leads us to two conclusions: (1) free trade is more likely within, rather than across, political–military alliances; and (2) alliances are more likely to evolve into free-trade coalitions if they are embedded in bipolar systems than in multipolar systems. Using data drawn from an 80-year period beginning in 1905, we test these hypotheses. The results of the analysis make it clear that alliances do have a direct, statistically significant, and large impact on bilateral trade flows and that this relationship is stronger in bipolar, rather than in multipolar, systems.*

In the early 1970s, students of international relations began what would become a long and heated debate about the political correlates of open international markets. Recently, however, an important element of consensus has begun to emerge: most contributors to this debate now seem to agree that the prisoner's dilemma structure of state preferences with respect to trade explains the relative dearth of open international markets.[1]

Here, we argue that what has become the standard prisoner's dilemma representation neglects the most critical aspect of free-trade agreements in the anarchic international system, namely, their security externalities. We consider these external effects explicitly. In order to do so, we construct and analyze a simple game-theoretic model. The results of this analysis demonstrate that tariff games between allies differ systematically from those played between actual or potential adversaries.

These differences imply that free trade is more likely within, rather than across, political–military alliances. However, our analysis also suggests that the evolutionary prospects of alliances vary: those that are the products of bipolar systems are more likely to evolve into free-trade coalitions than are their multipolar counterparts. Less credible exit threats and clearer responsibilities for alliance stability explain the advantage of a bipolar system.

We test this argument using data drawn from an 80-year period beginning in 1905. The empirical analysis supports our argument. It demonstrates that alliances have a direct, statistically significant, and large effect upon bilateral trade. Moreover, it shows that on average, alliances have a much stronger effect on trade in a bipolar than in a multipolar world.[2]

## POWER, TRADE, AND TARIFFS

Unlike many arguments about the use of economic statecraft, ours is cast at the macroeconomic rather than at the microeconomic level. In other words, the argument we advance here is *not* about, for example, attempts to use economic statecraft to embargo exports of particular products or to inhibit the development of technologically advanced industries in other countries.[3] Instead, it is an argument that is based upon the effect of free trade on the real income and power potential of states.

We argue that the play of power politics is an inexorable element of any agreement to open international markets because trade produces security externalities.[4] These externalities arise because the source of gains from trade is the increased efficiency with which domestic resources can be employed. This increased efficiency itself frees economic resources for military uses (Baldwin 1985, 216; Hirschman 1980, 14; McKeown 1982, 225; idem 1984, 232; Root 1984, 75; Srinivasan 1987, 352). As a consequence, trade enhances the potential military power of any country that engages in it.

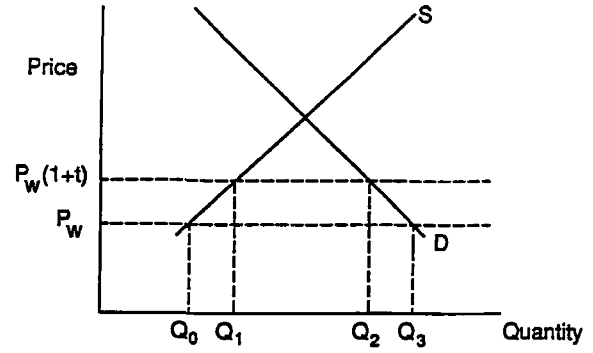
The anarchic structure of the international system, in turn, compels its constituent states to attend closely to the potential military power of both allies and potential or actual adversaries. It does so because the absence of any supranational authority in the international system enables a state to resort to force at any time to achieve its goals. The probability that a state will do so depends in part upon its power, which, in turn, depends partly upon its real income.

As a consequence, the real income gains that motivate free trade are also the source of the security externalities that can either impede or facilitate it: trade with an adversary produces a security diseconomy; trade with an ally produces a positive externality. In either case, agreements to open international markets create a divergence between the private and social costs of trade.[5] A socially suboptimal level of trade results, suggesting that government intervention in trade can be welfare-enhancing for the nation as a whole.

Figure 1 illustrates this situation. It shows the domestic market for a good that can also be imported at its world price, $P_w$, which is the private marginal

FIGURE 1

**Trade with an Adversary**



cost of imports. In the absence of government intervention, domestic output is $Q_0$; domestic demand is $Q_3$; and the amount imported is the difference between them ($Q_3 - Q_0$). Because of the external diseconomy that trade with actual or potential adversaries generates, however, the marginal social cost of imports is higher, $P_w(1 + t)$. This market failure can be eliminated by a tariff, $t$, which raises domestic output to $Q_1$, lowers domestic demand to $Q_2$, and reduces imports to $Q_2 - Q_1$.[6] Government intervention in trade to correct this distortion is, in addition, a first-best policy: the market failure in this instance does not originate in a domestic distortion but arises, instead, in trade itself.[7]

## OPTIMAL TARIFF GAMES

Successful intervention in trade to correct the security externalities associated with it depends upon the ability of a country to affect the real income of the state which is its target. Although any tariff will distort resource allocation in the target country and therefore decrease its real income, it will do so without imposing net costs on the home country only if the latter can affect its terms of trade.

Thus, in the absence of the requisite market power, a tariff will impose costs primarily upon the state that levies it. In its presence, a tariff will allow a state to increase its own real income at its adversary's expense. If a state can affect world prices, then, it can use a tariff to narrow the gap between the private and social costs of trade. Thus, a tariff can be welfare-superior to a policy of free trade for a state in an anarchic international political system.

This argument is, of course, a variant of the traditional optimal tariff argument. Its novelty inheres in the two conclusions that emerge from our discussion below: (1) tariff games between allies differ systematically from those played between adversaries; and (2) intraalliance free trade is more likely in a bipolar than in a multipolar system.

### The Standard Optimal Tariff Game

The payoff matrix of the standard optimal tariff game is shown in Figure 2. This game is a prisoner's dilemma in which T is the payoff that accrues to a state that unilaterally deploys an optimal tariff, R is the payoff that results if both states trade freely with each other, P is the return that accrues if both use optimal tariffs, and S is the payoff to unilateral free trade. As the matrix makes apparent, in a one-shot game, defection is a dominant strategy.[8] As a result, the inevitable outcome of this (or of a finite) game is bilateral tariffs.[9]

In contrast, in an infinite game or in a game in which states assume at every period that there is some positive probability that the game will continue, a variety of Pareto-improving outcomes, including that of free trade, can be realized.[10] This can occur if, for example, both states adhere to a "grim" strategy, that is, begin with free trade (i.e., cooperate); trade freely in every succeeding period if the outcome of the previous period was bilateral free trade; otherwise, deploy an optimal tariff (i.e., defect).[11]

This strategy will sustain cooperation in a prisoner's dilemma game if the discounted sum of cooperative payoffs, $R/(1 - \delta)$, is greater than the sum of the one-shot gain from defection, T, and the discounted sum of punishment payoffs, $\delta P/(1 - \delta)$.[12] Thus, cooperation can be sustained by a grim strategy if

FIGURE 2

**The Prisoner's Dilemma**

| | | Column | |
|---|---|---|---|
| | | Cooperate | Defect |
| Row | Cooperate | R,R | S,T |
| | Defect | T,S | P,P |

*Note*: T > R > P > S; Row's payoffs are listed first; $(T + S)/2 \leq R$.

**FIGURE 3**

**The Prisoner's Dilemma: Adversaries**

| | | State j | |
|---|---|---|---|
| | | Cooperate | Defect |
| State i | Cooperate | $R_i - w_{ij}R_j,\ R_j - w_{ji}R_i$ | $S_i - w_{ij}T_j,\ T_j - w_{ji}S_i$ |
| | Defect | $T_i - w_{ij}S_j,\ S_j - w_{ji}T_i$ | $P_i - w_{ij}P_j,\ P_j - w_{ji}P_i$ |

*Note*: This payoff matrix omits second-order effects.

$$R/(1 - \delta) \geq T + \delta P/(1 - \delta)$$

or

$$\delta \geq (T - R)/(T - P).$$

If this incentive compatibility constraint is satisfied, a grim strategy will enable free trade to emerge as the outcome of an infinite-horizon optimal tariff game. That this is possible not only in the abstract but also in the real world is suggested by the success of the General Agreement on Tariffs and Trade (GATT). Established in 1947, the GATT was created to, and did, enable states to avoid a replay of the tariff game that stymied trade among them in the interwar period.[13]

### Tariff Games Between Adversaries

The payoff matrix of the standard optimal tariff game assumes that there is no difference between private and social returns to interstate trade. Life in an anarchic international system, however, effectively alters the matrix of the standard game. Because interstate trade in such a system produces security externalities, it creates a divergence between private and social returns that any utility-maximizing state will take into account when it calculates its payoffs from trade.

Thus, the payoffs a state assigns to the outcomes of any given trade game differ from those of the standard optimal tariff game. In the case of trade with an adversary, a state incurs a marginal social cost that the standard matrix does not reflect. This transforms the standard matrix as shown in Figure 3. In each cell, the social cost that a state, i, incurs is represented as a fraction, $w_{ij}$, of the payoff that accrues to its adversary, j. Thus, $w_{ij}R_j$, for example, is the social cost or security diseconomy that accrues to state i when it trades freely with state j.

This cost is represented as an increasing function of the adversary's gains from trade. This representation is based in part on what Robert Powell has described as a "very simple, highly stylized assumption about the nature of warfare": that is, the "stronger a state is economically, the more likely it is to prevail in war" (1991, 1312). The functional dependence of social on private returns from trade also has a sector-specific component. The increase in domestic resource efficiency that results from trade frees economic resources for military uses and, therefore, increases an adversary's actual or potential military power.

The marginal social cost of trade is represented as a fraction, $w_{ij}$, of the adversary's gains from trade, partly because military power obviously does not depend only upon gross national product. It also depends, for example, on technology, skill, and the extractive potential of the state. In addition, military portfolios reflect the diversified interests of great powers. Because military resources are not completely fungible, not all additions to the military power of an adversary will weaken its trading partner. Thus, even if an adversary did divert all of its gains from trade to its military sector, its action would not necessarily inflict a social cost on its trading partner equal to the incremental addition to its military power.

When the returns from trade are adjusted to reflect its marginal social cost, the net effect is to make tariff games between adversaries more difficult to solve than is the standard optimal tariff game. Assuming that the game is iterated, free trade can be sustained in an optimal tariff game between adversaries if

$$(R_i - w_{ij}R_j)/(1 - \delta_i) \geq T_i - w_{ij}S_j + \delta_i\,(P_i - w_{ij}P_j)/(1 - \delta_i)$$

or

$$\delta_i^* \geq \frac{T_i - w_{ij}S_j - (R_i - w_{ij}R_j)}{T_i - w_{ij}S_j - (P_i - w_{ij}P_j)}.$$

This condition is more difficult to satisfy than the condition that emerges from an optimal tariff game among states with independent utility functions. In a prisoner's dilemma game with payoffs of (3, 2, 1, 0), for example, the minimum discount factor that would support free trade in the standard game is .50. In the modified game (with $w$ = .40), the analogous figure rises to .75.[14] Thus, the introduction of interdependent utility functions characteristic of adversarial states makes any given prisoner's dilemma more "severe" or difficult to solve. In addition, as $w$ increases, the severity of the game also increases.[15]

FIGURE 4

**The Prisoner's Dilemma: Allies**

| | | State j | |
|---|---|---|---|
| | | Cooperate | Defect |
| State i | Cooperate | $R_i + w_{ij}R_j, R_j + w_{ji}R_i$ | $S_i + w_{ij}T_j, T_j + w_{ji}S_i$ |
| | Defect | $T_i + w_{ij}S_j, S_j + w_{ji}T_i$ | $P_i + w_{ij}P_j, P_j + w_{ji}P_i$ |

*Note*: This payoff matrix omits second-order effects.

## Tariff Games Between Allies

The gains of one state impose social costs on another only if the former is an actual or potential threat to the security of the latter. When a state trades with an ally, however, it realizes both private and social benefits. In the case of allies, the security externality that trade produces is positive. As in the case of trade between adversaries, a utility-maximizing state will take account of not only private but also social returns in calculating its payoffs from trade.

Thus, the standard matrix does not apply to this case either. The transformed prisoner's dilemma matrix is shown in Figure 4. In each cell, the social benefit that that a state, i, realizes as a consequence of trade with an ally is represented as some fraction, $w_{ij}$, of the payoff that accrues to its ally, j. This effectively internalizes the positive security externality associated with free trade between allies. The functional form is based on reasoning analogous to that which applies in the case of trade between adversaries. The applicable incentive compatibility constraint then becomes

$$(R_i + w_{ij}R_j)/(1 - \delta_i) \geq T_i + w_{ij}S_j + \delta_i\ (P_i + w_{ij}P_j)/(1 - \delta_i)$$

or

$$\delta_i^{**} \geq \frac{T_i + w_{ij}S_j - (R_i + w_{ij}R_j)}{T_i + w_{ij}S_j - (P_i + w_{ij}P_j)}\,.$$

This condition is more easily satisfied than is the condition that emerges from either the standard prisoner's dilemma or the transformed game among adversaries. Using the same values assigned in the example above, free trade can be sustained among allies when the discount factor is .13. In addition, if $w$ exceeds the larger of $(T - R)/(T - S)$ and $(D - S)/(T - S)$, free trade becomes the dominant strategy equilibrium. In short, tariff games between allies are easier to solve than tariff games between actual or potential adversaries.

## The Influence of Polarity

While this model suggests that all alliances will influence trade barriers, it seems clear that alliances have exerted a stronger effect after, than before, World War II. Inspection of the incentive compatibility constraint that applies to the intraallied tariff game suggests one explanation of this difference. All other things being equal, the extent to which allies trade freely with each other depends upon the discount factor, $\delta$. The discount factor is a function of the risk of exit, which, in turn, is a function of systemic polarity (Gowa 1989).[16]

The risk of exit is the threat that an ally will abandon an existing alliance to join an alternative one. For several reasons, this risk is higher in multipolar than in bipolar systems.[17] First, while bipolar coalitions are the products largely of system structure, alliances in a multipolar system are the results of choice among several possible alternatives (Snyder 1984, 415). Second, in a bipolar system, realignment is impossible for either pole. By default, alliance stability is also the exclusive responsibility of each. Neither can expect any other state to prevent the defection of an ally from within its bloc (Snyder 1984). In a multipolar system, however, the interest in preserving alliance stability can be distributed across more than one pole. As a consequence, alliance stability can become problematic, as each pole seeks to transfer the burden of maintaining the alliance to another.

The implications of different exit risks for trade are clear. Any great-power member of a coalition in a multipolar world has strong incentives to discriminate among its allies in terms of its investment in them, because its allies confront divergent opportunity costs of exit. In contrast, the incentive to discriminate in a bipolar world is much weaker, since allies are much more uniformly and securely locked into coalitions. In a bipolar world, in other words, the gains from trade can be more easily privatized. As a consequence, investments in allies in a multipolar system are likely to be expressed in the form of discriminatory trade preferences; in a bipolar system, free trade is the more likely outcome.

In short, because the risk of exit is lower in a bipolar than in a multipolar system, the security externalities of any free-trade agreement are more likely to remain internalized within the alliances of the former than in those of the latter. As a result, allies in a multipolar system will tend to discount the future benefits accruing from open markets among them more heavily than will their bipolar counterparts. The greater stability of bipolar coalitions allows the value of future benefits to approximate present benefits more closely.

For any given structure of payoffs, then, free trade is more likely to emerge within the alliances of a bipolar than in those of a multipolar system. It is not surprising, therefore, that the effect of alliances on trade seems to have been greater after rather than before World War II. We shall now subject this and our more general hypothesis about the impact of alliances on trade to a systematic empirical test.

## THE RESEARCH DESIGN

Empirical analyses of the influence of political factors on bilateral trade are relatively rare. However, because several important studies do address closely related topics, we want to examine them briefly here. One group of studies examines the effects on bilateral trade of preferential trading arrangements (PTAs), such as customs unions and free-trade areas. Many of these analyses conclude that PTAs have done much to promote trade among member nations (Aitken 1973; Linnemann 1966; Pelzman 1977; Tinbergen 1962). Another group of studies uses events data to analyze the impact on bilateral trade of political conflict and cooperation. These analyses conclude that bilateral trade tends to be lower when the participants are engaged in conflicts than when political relations between them are relatively cooperative (Pollins 1989a, 1989b).

Our analysis differs from these studies in three ways. First, clear differences exist between alliances and economic arrangements that are explicitly designed to promote trade among their members. Moreover, since many PTAs have formed among political allies (Mansfield 1993), our model may at least partially endogenize their effects on bilateral trade. Second, it is unlikely that a very strong relationship exists between alliances and measures of conflict and cooperation based on events data: there may be a considerable degree of low-level conflict among members of an alliance and substantial cooperation among states that are not allied. Third, these studies examine the determinants of bilateral trade after 1945. To adequately test our model, we need to consider a much longer time period. Hence, while previous analyses have yielded important insights into the effects of certain aspects of international politics on trade, they do not analyze either the same dimension of international politics or the same period of time as we do here.[18]

Based on our argument, we expect that (1) fewer trade barriers will exist within than across alliances; and (2) intraalliance free trade will be more likely within bipolar than within multipolar systems. While we would prefer to test these hypotheses directly, it is not possible to do so. Reliable tariff data do not exist for all countries over the entire time span that we analyze. However, to the extent that alliance patterns explain tariffs and that tariffs, in turn, explain the volume of trade, tariffs are endogenized, and we can focus on the relationship between alliances and trade.[19] As a result, we examine the effects of alliances on bilateral trade flows.

Ideally, we would include in our sample all major powers and their allies. However, data limitations led us to focus on trade relations among the United States, Great Britain, France, Italy, Germany, the Soviet Union,[20] and Japan during the period from 1905 to 1985. Though this is clearly a limited sample, these states include most of the major powers in the international system during this period (Levy 1983; Small and Singer 1982). Moreover, since we expect the impact of alliances on trade to be most pronounced in cases of alliances involving major powers, this analysis will offer an important, though preliminary, test of our model.[21]

We also expect that bilateral alliances will affect trade flows more strongly than will multilateral alliances. While the *aggregate* flow of trade across a group of more than two states may be higher when these states are engaged in an alliance than when they are not, this need not imply that *all* bilateral trade flows among these states will be higher under these circumstances. For example, the existence of an alliance among states A, B, C, and D would be expected to produce an increase in the total trade among these states. This, however, would not necessarily lead to an increase in bilateral trade between A and B. Indeed, A and B may engage in less commerce during the course of this alliance than in the absence of the alliance (although they would be expected to offset this by trading more with C and/or D). But if our argument holds, bilateral alliances should be directly related to bilateral trade. Under these circumstances, the effects of alliances on aggregate intraalliance trade can be assessed directly by examining trade flows between the two partners.

## THE MODEL

In this section, we develop a simple model of the relationship between alliances and bilateral trade. It is clear that any analysis of this sort should incorporate economic, as well as political, determinants of trade. Among economists, gravity models of bilateral trade flows have been widely used (Aitken 1973; Anderson 1979; Deardorff 1984, 503–4; Frankel 1992; Leamer and Stern 1970, 145–70; Linnemann 1966; Pelzman 1977; Tinbergen 1962). In order to analyze the economic determinants of bilateral trade, we use a well-known variant of this model, which includes the gross national product (GNP) and the population of both the importer and the exporter, as well as the geographic distance between the two states. Consistent with previous research based on this model, we expect that the nominal value of bilateral trade will be directly related to the nominal GNP of both the importer and the exporter and inversely related to the population of both the importer and exporter and to the geographic distance between them.

In addition to GNP, population, and distance, we include two variables related to alliances in our model: one indicating whether a bilateral alliance exists between the trading partners and one indicating whether a multilateral alliance exists between these states. Finally, because we expect interstate wars to reduce trade among the belligerents, we also include a variable that indicates whether or not the importer and exporter are at war.

Hence, our model is

$$\begin{aligned}\log X_{ij(t)} &= \log A + B_1 \log Y_{i(t-1)} + B_2 \log Y_{j(t-1)} \\ &+ B_3 \log P_{i(t-1)} + B_4 \log P_{j(t-1)} \\ &+ B_5 \log D_{ij(t-1)} + B_6 \log BA_{ij(t-1)} \\ &+ B_7 \log MA_{ij(t-1)} + B_8 \log War_{ij(t-1)} + \log z_{ij}, \quad \textbf{(1)}\end{aligned}$$

where $\log X_{ij(t)}$ is the natural logarithm of the nominal value of exports (expressed in U.S. dollars) by state i to state j in year t,[22] $\log Y_{i(t-1)}$ is the natural logarithm of the nominal GNP (expressed in U.S. dollars) of state i in year $t - 1$,[23] $\log Y_{j(t-1)}$ is the natural logarithm of the nominal GNP (expressed in U.S. dollars) of state j in year $t - 1$, $\log P_{i(t-1)}$ is the natural logarithm of the population of state i in year $t - 1$,[24] $\log P_{j(t-1)}$ is the natural logarithm of the population of state j in year $t - 1$, $\log D_{ij(t-1)}$ is the natural logarithm of the geographic distance between states i and j in year t,[25] $\log BA_{ij(t-1)}$ is a dummy variable that equals 1 if an alliance exists between states i and j in year $t - 1$ that is comprised of no members except i and j (and 0 otherwise),[26] $\log MA_{ij(t-1)}$ is a dummy variable that equals 1 if an alliance exists between states i and j in year $t - 1$ that includes at least one additional member (and 0 otherwise), $\log War_{ij(t-1)}$ is a dummy variable that equals 1 if states i and j are engaged in a war in year $t - 1$ (and 0 otherwise),[27] and $\log z_{ij}$ is an error term. Note that in antilogarithmic form all dummy variables in this model take on values of e (the base of the natural logarithms) and one; the natural logarithms of these variables, therefore, take on values of one and zero. Since it is generally assumed that these variables exert a lagged effect on the value of exports and in order to avoid problems of simultaneity,[28] we assume a lag of one year in equation 1 for each variable. The log-linear specification of this model is used because it is consistent with many previous studies of trade that have used gravity models and because it has number of advantages relative to a linear specification (Leamer and Stern 1970).

Since we are interested in the relationship between alliances and trade at given points in time, as well as over time, equation 1 is estimated for a series of cross sections, beginning in 1905. Our analysis begins with 1905 (year t) because complete data for all of the independent variables in equation 1 are not available prior to 1904 (year $t - 1$). After 1905, the parameters in equation 1 are estimated for the first year of each subsequent ten-year interval. However, we do not estimate the model during World Wars I and II, since trade data are often not available for these years. Further, equation 1 is not estimated during the late 1940s because the occupation of Germany and Japan precluded both states from making autonomous decisions about trade policies or alliance partners. As a result, there are some cases in which the intervals are not ten years in length.

## Estimates of the Parameters

Ordinary least squares estimates of the parameters in equation 1 are presented in Table 1. In each regression, at least one outlier was identified.[29] The results in Table 1 are estimated without these observations. However, it should be noted that very little difference exists between the results with these observations included and the results presented in Table 1.[30]

These findings indicate that on average, our model explains over 80% of the variation in the value of exports and that the overall fit of the model differs little across the nine years that are analyzed. Our results also indicate that the regression coefficients of GNP, population, geographic distance, and war usually point in the expected directions and are statistically significant. First, the regression coefficient of log $Y_i$ is positive in nine cases and statistically significant in eight instances; and the regression coefficient of log $Y_j$ is positive and statistically significant in all nine cases. Second, the regression coefficient of log $P_i$ is negative in eight cases and statistically significant in seven instances; and the regression coefficient of log $P_j$ is negative in nine cases and statistically significant in eight instances. Third, the regression coefficient of log $D_{ij}$ is negative in eight cases and statistically significant in five instances. Finally, the regression coefficient of log $War_{ij}$ is negative and statistically significant in one out of two cases.

These results also confirm the hypothesis that alliances are directly related to the value of exports. The regression coefficient of log $BA_{ij}$ is positive in six out of nine cases and statistically significant in five instances; and the regression coefficient of log $MA_{ij}$ is positive and statistically significant in six out of eight cases. Further, the mean of the nine regression coefficients of log $BA_{ij}$ is about 1.09 and (assuming that these regression coefficients are statistically independent) the standard error of the mean is about .15.[31] The mean of the eight regression coefficients of log $MA_{ij}$ is about .61, and the standard error of the mean is about .16.[32] On average, therefore, the relationships between bilateral alliances and multilateral alliances, on the one hand, and bilateral trade on the other seem to be direct and statistically significant.

Moreover, the quantitative impacts of both bilateral and multilateral alliances on the predicted value of bilateral trade are substantial. Even if the mean of the regression coefficients of log $BA_{ij}$ was two standard errors less than 1.09, a change from the absence of a bilateral alliance to the existence of such an alliance would more than double the predicted value of exports.[33] And even if the mean of the regression

TABLE 1

Regression of Exports on GNP, Population, Distance, Alliances, and War, 1905–85

| PARAMETER | PERIOD OF MULTIPOLARITY | | | | | PERIOD OF BIPOLARITY | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 1905 | 1913 | 1920 | 1930 | 1938 | 1955 | 1965 | 1975 | 1985 |
| Intercept | −4.57 | −8.79 | 57.21*** | 7.39 | 12.44* | 34.81*** | 5.69 | 6.29 | 12.14** |
| | (7.88) | (9.99) | (14.74) | (5.06) | (6.19) | (8.19) | (5.17) | (4.25) | (4.88) |
| log GNP$_i$ | .95*** | 1.68*** | 2.78*** | 1.53*** | 1.67*** | 1.12*** | .28 | .83*** | .96*** |
| | (.17) | (.23) | (.34) | (.14) | (.20) | (.26) | (.26) | (.20) | (.32) |
| log GNP$_j$ | 1.10*** | .90*** | 2.17*** | 1.25*** | 1.57*** | .93*** | .44** | .55*** | 1.19*** |
| | (.18) | (.25) | (.27) | (.14) | (.22) | (.25) | (.26) | (.21) | (.32) |
| log Population$_i$ | −.02 | −.95*** | −4.10*** | −1.21*** | −1.68*** | −1.88*** | .14 | −.68*** | −1.13*** |
| | (.33) | (.35) | (.83) | (.23) | (.34) | (.49) | (.39) | (.26) | (.42) |
| log Population$_j$ | −1.21*** | −.92*** | −4.83*** | −1.74*** | −2.27*** | −1.82*** | −.38 | −.45** | −1.42*** |
| | (.27) | (.31) | (.59) | (.23) | (.32) | (.49) | (.39) | (.25) | (.40) |
| log Distance$_{ij}$ | −.33*** | −.06 | .27 | −.35*** | −.06 | −.01 | −.12** | −.23*** | −.28*** |
| | (.09) | (.10) | (.13) | (.06) | (.09) | (.09) | (.06) | (.05) | (.07) |
| log Bilat. alliance$_{ij}$ | −.37 | −.57 | .20 | 1.04*** | −.30 | 3.02*** | 2.58*** | 2.07*** | 2.10*** |
| | (.31) | (.36) | (.77) | (.42) | (.40) | (.55) | (.39) | (.32) | (.43) |
| log Multilat. alliance$_{ij}$ | −.61 | −.31 | .96*** | —[a] | .48* | .86** | 1.65*** | .99*** | .84*** |
| | (.55) | (.72) | (.39) | | (.35) | (.46) | (.33) | (.18) | (.26) |
| log War$_{ij}$ | −7.12*** | —[b] | 1.55 | —[b] | —[b] | —[b] | —[b] | —[b] | —[b] |
| | (.61) | | (.72) | | | | | | |
| Adjusted $R^2$ | .92 | .71 | .83 | .86 | .80 | .78 | .82 | .82 | .80 |
| N | 39 | 39 | 37[c] | 40 | 37[d] | 41 | 40 | 41 | 41 |

*Note*: Entries are unstandardized regression coefficients with standard errors in parentheses. Years shown are year t in equation 1. For each year, there are 42 observations minus the number of outliers.

[a] No multilateral alliances existed among the major powers in 1929.

[b] No wars between major powers were conducted during these years.

[c] No data on the Soviet Union's exports to Germany are available for 1920.

[d] No data on Italian exports to the Soviet Union are available for 1938.

*$p \leq .10$ (one-tailed test); intercept $p \leq .10$ (two-tailed test).

**$p \leq .05$ (one-tailed test); intercept $p \leq .05$ (two-tailed test).

***$p \leq .01$ (one-tailed test); intercept $p \leq .01$ (two-tailed test).

coefficients of log MA$_{ij}$ was two standard errors less than .61, a change from the absence of a multilateral alliance to the existence of such an alliance would increase the predicted value of exports by more than one-third.[34]

As expected, the system's structure influences the strength and the magnitude of the effect of alliances on trade. The means of the regression coefficients of log BA$_{ij}$ and log MA$_{ij}$ have been substantially larger during periods of bipolarity than during periods of multipolarity; and the differences between these means are statistically significant (for log BA$_{ij}$, t = 6.26, $p < .005$; for log MA$_{ij}$, t = 2.34, $p < .05$). Thus, our results indicate that on average, the magnitude of the effects of alliances on trade has been considerably more pronounced during bipolar periods than during multipolar periods.

There is also some support for our hypothesis that bilateral alliances exert larger effects on trade than multilateral alliances, although the evidence is weaker than expected. When all years in Tables 1 are considered, the mean of the regression coefficients of log BA$_{ij}$ is almost twice as large as the mean of the regression coefficients of log MA$_{ij}$, but the difference between these means is not statistically significant. When only bipolar periods are examined, the mean of the regression coefficients of log BA$_{ij}$ is substantially larger than the mean of the regression coefficients of log MA$_{ij}$; and the difference between these means is statistically significant (t = 4.50, $p < .005$). But when only multipolar periods are analyzed, the mean of the regression coefficients of log MA$_{ij}$ is somewhat larger than the mean of the regression coefficients of log BA$_{ij}$; and, although the difference between these means is not statistically significant, this result is clearly at odds with our hypothesis. Hence, the extent to which our hypothesis concerning the effects of different types of alliances on trade is supported seems to depend in large measure on system structure.

## THE ROBUSTNESS OF THE RESULTS

In addition to estimating the effects of alliances on trade, it is also important to analyze the robustness of the results in Table 1. To this end, we address two issues. First, does the composition of the sample of states affect the results? More specifically, does the fact that we include the Soviet Union in our sample

TABLE 2

**Regression of Exports on Per Capita GNP, Distance, and Alliances, Excluding the Soviet Union, 1905–1985**

| | PERIOD OF MULTIPOLARITY | | | | PERIOD OF BIPOLARITY | | | |
|---|---|---|---|---|---|---|---|---|
| PARAMETER | 1905 | 1913 | 1920 | 1938 | 1955 | 1965 | 1975 | 1985 |
| Intercept | 9.57***<br>(2.54) | 3.60<br>(3.46) | 2.68<br>(2.95) | 3.17<br>(3.05) | 10.51***<br>(1.85) | 8.73**<br>(3.48) | 8.33***<br>(2.79) | 7.78*<br>(4.27) |
| log Per capita GNP$_i$ | 1.12***<br>(.23) | 1.76***<br>(.30) | 1.88***<br>(.29) | 1.41***<br>(.26) | .80***<br>(.21) | .79***<br>(.31) | .90***<br>(.21) | .69**<br>(.30) |
| log Per capita GNP$_j$ | .96***<br>(.23) | .69***<br>(.32) | .94***<br>(.28) | 1.36***<br>(.26) | .51***<br>(.20) | .82***<br>(.30) | .78***<br>(.21) | 1.15***<br>(.30) |
| log Distance$_{ij}$ | −.26**<br>(.11) | .06<br>(.14) | −.21**<br>(.10) | −.18**<br>(.10) | −.14*<br>(.10) | −.17*<br>(.11) | −.21***<br>(.06) | −.32***<br>(.09) |
| log Bilat. alliance$_{ij}$ | −.41<br>(.39) | −.11<br>(.42) | .58<br>(.66) | −.21<br>(.62) | 2.36***<br>(.49) | 2.41***<br>(.51) | 2.18***<br>(.32) | 1.92***<br>(.44) |
| log Multilat. alliance$_{ij}$ | −.61<br>(.63) | −.12<br>(.75) | 1.06**<br>(.35) | .33<br>(.41) | 1.31***<br>(.53) | .93*<br>(.55) | .97***<br>(.25) | .80***<br>(.31) |
| Adjusted $R^2$ | .75 | .63 | .74 | .78 | .87 | .80 | .85 | .81 |
| N | 28 | 28 | 28 | 28 | 29 | 30 | 28 | 28 |

*Note*: Entries are unstandardized regression coefficients with standard errors in parentheses. Years shown are year t in equation 2. For each year, there are 30 observations minus the number of outliers.
*$p \leq .10$ (one-tailed test); intercept $p \leq .10$ (two-tailed test).
**$p \leq .05$ (one-tailed test); intercept $p \leq .05$ (two-tailed test).
***$p \leq .01$ (one-tailed test); intercept $p \leq .01$ (two-tailed test).

enhance the influence of alliances on trade? Second, does the omission of certain variables that might be expected to influence both alliance patterns and trade flows account for the observed relationship between alliances and bilateral commerce?

### Estimates of the Parameters Excluding the Soviet Union

It is possible that including the Soviet Union in our sample may have exaggerated the impact of alliances on trade (in Table 1). The Soviet Union had both a command and a relatively autarkic economy during most of the twentieth century. It also concluded few alliances with other major powers in our sample after World War I. Thus, the strength of the observed relationship between alliances and trade might be due to the composition of our sample. In order to determine whether this is the case, ordinary least squares estimates of the parameters in equation 1 were obtained for those dyads that did not involve the Soviet Union.

These results (and auxiliary regressions based on these results) indicated that unlike our findings in Table 1, there was often considerable multicollinearity among log $Y_i$, log $Y_j$, log $P_i$, and log $P_j$ when dyads involving the Soviet Union were excluded from our analysis. One solution to this type of problem is to combine some of the explanatory variables (Fox 1991, 15–16). We, therefore, focus on the effects of the nominal per capita GNP (which is expressed as Y/P) of both the importer and the exporter, as well as distance and alliances, on nominal exports. This specification of our model is as follows:

$$\log X_{ij(t)} = \log A + B_1 \log (Y/P)_{i(t-1)} + B_2 \log (Y/P)_{j(t-1)} + B_3 \log D_{ij(t-1)} + B_4 \log BA_{ij(t-1)} + B_5 \log MA_{ij(t-1)} + \log z_{ij}, \quad (2)$$

where log $(Y/P)_{i(t-1)}$ is the natural logarithm of the nominal per capita GNP of state i in year $t - 1$, log $(Y/P)_{j(t-1)}$ is the natural logarithm of the nominal per capita GNP of state j in year $t - 1$, and the nominal per capita GNP of both states i and j is expected to be directly related to the nominal value of exports from i to j (Frankel 1992, Linnemann 1966). Since each of the wars we considered in our earlier analysis involved the Soviet Union, War$_{ij}$ is not included in this model. Further, because all alliances in 1929 involved the Soviet Union, it was not possible to estimate the effects of alliances on trade for this year.

Ordinary least squares estimates of the parameters of equation 2 are presented in Table 2 (with outliers deleted).[35] These results indicate that on average, our model continues to explain about 80% of the variation in exports and that the signs of the regression coefficients of per capita GNP and geographic distance are in the expected directions. In every case, the regression coefficients of log $(Y/P)_i$ and log $(Y/P)_j$ are positive and statistically significant; and the regression coefficient of log $D_{ij}$ is negative and statistically significant in seven out of eight instances.

Of particular interest is the fact that the effects of both bilateral and multilateral alliances on bilateral trade are much the same, regardless of whether or not the Soviet Union is excluded from our sample of states. In no case does the sign of the regression coefficient of either log $BA_{ij}$ or log $MA_{ij}$ in Table 1 change when the Soviet Union is deleted; and in only one case (log $MA_{ij}$ in 1938) does the level of statistical significance of the regression coefficient of either log $BA_{ij}$ or log $MA_{ij}$ in Table 1 change when the composition of our sample of states is altered. Moreover, the means of the regression coefficients of both log $BA_{ij}$ (1.09) and log $MA_{ij}$ (.58) are virtually identical to those based on the results in Table 1.

On average, therefore, the relationships between both bilateral and multilateral alliances and bilateral trade continue to be direct and statistically significant. It should also be noted that on average, the magnitude of the effects of bilateral alliances on trade continues to be larger than that of multilateral alliances and that on average, the magnitude of the effects of both bilateral and multilateral alliances on bilateral trade continues to be significantly larger during periods of bipolarity than during periods of multipolarity.

### The Effects of Omitted Variables

We now turn to the issue of whether our results in Table 1 are robust with respect to the inclusion of variables that are omitted from equation 1 and that might account for the observed relationship between alliances and trade. First, as we have noted, many analysts have concluded that PTAs have promoted trade among their members. Further, since many of these arrangements have been embedded in alliances, it may be that PTAs are responsible for the observed relationship between alliances and trade.

In order to test this hypothesis, we conducted two separate analyses. Initially, we included a dummy variable in equation 1 that equalled 1 if states i and j were members of the European Economic Community in year $t - 1$, and 0 otherwise. Next, we included a dummy variable in equation 1 that equalled 1 if states i and j were members of the GATT in year $t - 1$, and 0 otherwise. In each case, we analyzed 1965, 1975, and 1985; and both analyses yielded quite similar findings.[36] In each analysis, the regression coefficient of the dummy variable was positive in all three cases (i.e., more trade has been conducted between states that were PTA members than between states that were not). But, in each case, only one regression coefficient was statistically significant; and the inclusion of this variable did not have a substantial effect on either the size or the level of statistical significance of any of the remaining regression coefficients in equation 1.

Second, in addition to wars, military disputes that do not escalate into wars may also influence bilateral trade relations. Further, the existence of military disputes may influence alliance patterns. In order to test whether military disputes account for the observed relationship between alliances and trade, we included in equation 1 a dummy variable that equalled 1 if states i and j were involved in a militarized dispute in year $t - 1$, and 0 otherwise. Among the years listed in Table 1, four were marked by military disputes, based on data developed by Gochman and Maoz (1984) and recently updated by the Correlates of War Project. Our findings indicated that the regression coefficient of this variable tended to be negative (i.e., the existence of a dispute involving states i and j tended to depress trade between them). But in no case was it statistically significant, nor did its inclusion in equation 1 substantially change either the size or the level of statistical significance of any of the remaining regression coefficients in our model.

Third, whether or not the states in our sample are democratic polities may account for the observed relationship between alliances and bilateral trade flows. Democratic states might, for example, trade more freely with each other than do either nondemocracies with democracies or nondemocracies with each other. This might occur because democratic states typically have relatively free market economies. Recent research also suggests that democracies have exhibited a disproportionate tendency to ally with one another during portions of the twentieth century (Siverson and Emmons 1991).

In order to determine whether the observed relationship between alliances and trade is due to regime type, a dummy variable was included in equation 1 that equalled 1 if both states i and j were democracies in year $t - 1$, and 0 otherwise, based on Doyle's (1986) data. Our results indicated that the sign of this regression coefficient varied substantially from year to year and that it was statistically significant in only one case. Moreover, the size and level of statistical significance of the remaining regression coefficients in equation 1 continued to be much the same as in Table 1.

Clearly, we are not suggesting that PTAs, military disputes, and regime type do not influence bilateral trade flows at all. But these factors do not seem to account for the observed relationship between alliances and trade among major powers during the twentieth century. Nor does the composition of the sample: our findings are also quite robust with respect to whether the Soviet Union is included in the analysis. Thus, while tentative, our results indicate that bilateral trade flows between major powers tend to be considerably higher when they are allies than when they are not.

## CONCLUSION

We have argued that a free-trade policy can be suboptimal for states in an anarchic international system. Under some conditions, an effort to induce trade to follow the flag can be welfare-enhancing. The results of our empirical analysis support the implications of our argument about the relationship between alliances and trade. They show that alliances exert a direct, statistically significant, and large effect on bilateral trade flows. They also show that the effects of alliances vary. Bilateral alliances sometimes influence trade flows more strongly than do multilateral alliances; and alliances embedded in bipolar systems have stronger effects upon trade than do their counterparts in multipolar systems.

The argument we develop here also suggests that there may be other sources of cross-alliance variation that our empirical analysis could not detect. One plausible source is differences across alliances with respect to the gains that would accrue from free trade and its associated externalities. According to the tenets of the Heckscher–Ohlin–Samuelson model of international trade, for example, gains from trade and differences in relative factor endowments are directly related. Thus, this model implies that allies with very different factor endowments will confront stronger incentives to trade freely with each other than will allies with more similar endowments.

The weight a state assigns to its ally will also affect the magnitude of the security externality that free trade generates. Cross-alliance variations in $w_{ij}$ might occur, for example, because of differences in the probability of war, the technology of weapons production, or the need (if any) for an ex ante coordination of forces. In addition, the magnitude of the external effects of trade depends upon the transaction costs of opening borders to trade. These costs might vary as a consequence, for example, of whether an alliance has been given formal institutional expression (Keohane 1984; Snidal 1985).

Because of these and other possible sources of cross-alliance variation, it is clear that we have not offered an exhaustive analysis of the impact of alliances on trade. Nonetheless, we have established the conditions under which it makes sense for states to attempt to tie trade to the flag. In addition, we show that alliances have influenced bilateral trade flows not only after, but also before, 1945. In doing so, we suggest that the end of the Cold War will precipitate changes in the play of power politics both in the political–military sphere and in the pattern of international trade.

## Notes

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1. Disagreement continues about the source of this preference ranking, however. Some attribute it to the public good character of international trade (e.g., Kindleberger 1973); others attribute it to the ability of states to exercise monopoly or monopsony power in world markets (Conybeare 1984). Still others contend that the trade game can be a coordination game (Snidal 1985).

2. Our argument and analysis are limited to the influence on trade of system-level variables. For incisive analyses of the domestic determinants of tariffs, see Frieden 1988; Magee, Brock, and Young 1989; and Ruggie 1982.

3. The voluminous literature on economic statecraft at the microeconomic level that now exists partly explains our decision to focus this analysis at the macroeconomic level. The decision, however, was also the product of the almost insuperable obstacles that confront any attempts at embargo, probably the most common form of microeconomic statecraft. For a good discussion, see Bayard, Pelzman, and Lopez 1983.

4. Externalities are uncompensated costs or benefits that an agent inflicts or confers on third parties.

5. This divergence arises because those whose actions generate externalities have no incentive to take these external effects into account. Thus, if an individual's actions produce diseconomies, for example, he will engage in them to a socially suboptimal extent. For a more detailed explanation, see Yeager and Tureck 1983/84, 661.

6. We use tariffs here as a substitute for several forms of government intervention in trade. Because of constraints imposed by the General Agreement on Tariffs and Trade, government intervention in trade now usually takes the form of nontariff barriers. As long as the home country captures the scarcity rents that accrue from trade barriers, the form in which intervention occurs does not affect the argument presented here.

7. The theory of the second best applies to most national security arguments for government intervention in trade: the latter usually imposes higher costs on national welfare than do policies that target the distortion directly. For example, if production in an import-competing industry is judged inadequate on national security grounds, the Pareto-optimal policy is a production subsidy (Corden 1986). The general principle, as Deardorff and Stern observe, "is that trade intervention, by introducing two distortions rather than one, may succeed in solving one problem but only at the same time that it causes another. Trade policy is like doing acupuncture with a fork: no matter how carefully you insert one prong, the other is likely to do damage" (1987, 39).

8. That is, a player is better off defecting regardless of what the other player does.

9. If incomplete information of a specific kind exists in a finite prisoner's dilemma game, however, it is possible for mutual cooperation to emerge on at least some plays of the game. See Kreps et al. 1982.

10. As the Folk theorem states, if the horizon is infinite and the actors are very patient, *any* individually rational outcome can be sustained (Fudenberg and Maskin 1986).

11. In a context in which deviations are perfectly observable, maximal punishment makes sense, because it never occurs on the equilibrium path. It is, therefore, costless (Tirole 1988, 252). Moreover, unlike Tit for Tat, a grim strategy is subgame perfect (i.e., it is rational for the players to follow the specified strategies if any defection occurs) (Rasmusen 1989, 91).

It is not, however, renegotiation-proof (i.e., if one player defects and the players have the opportunity to renegotiate, they will abandon the punishment strategy in favor of one that confers higher payoffs.) A Pareto-perfect or weakly renegotiation-proof strategy is a "penance" strategy: begin with cooperation; if a deviation occurs, the deviator cooperates and the other player defects; the players then revert to cooperation (Fudenberg and Tirole 1991, 179–82). We use a grim strategy here for two reasons: (1) it is more accessible; and more importantly, (2) our results do not change if a penance strategy is used.

12. The discount factor, $\delta$, is "the value in present payoff units of one payoff unit to be received one period from the present." It reflects both the rate of time preference, $\rho$, and the probability that the game will end, $\Theta$. The discount factor is $(1 - \Theta)/(1 + \rho)$ (Rasmusen 1989, 90). Thus, the discounted sum of an infinite series of payoffs, x, received at the beginning of each period is: $(x)\ (1 - \Theta)/(1 + \rho)$.

13. Even though much progress has been made, com-

pletely free trade remains elusive. Nonetheless, the GATT has clearly facilitated the process of lowering trade barriers among its member countries. Yarbrough and Yarbrough (1992) argue, however, that it is not grim strategies but third-party (i.e., U.S.) enforcement that has deterred states from defecting from GATT agreements. For a dramatic comparison of the postwar and interwar periods, see Oye 1992.

14. Adapted from Snidal 1991.

15. The game will remain a prisoner's dilemma as long as $0 \leq w < 1$.

16. We distinguish bipolar and multipolar systems here in terms of the number of great powers that exist in any given system (Snyder and Diesing 1977, 419–20; Waltz 1979). We do not mean to imply, however, that polarity is either the only, or necessarily the most important, aspect of the distribution of power. Other aspects, especially the concentration of power, might also influence the relationship between alliances and trade (Mansfield n.d.).

17. Empirical analyses support this assertion. See, e.g., Duncan and Siverson 1982.

18. It should be noted that Pollins's (1989a, 1989b) analyses take account of the effects of alliances on trade to the extent that a "major strategic alliance" is considered a cooperative "event" in the data that he uses (see Azar 1980; Polachek 1980, 58, n. 5). However, since Pollins's objective is to assess the effects of aggregate levels of conflict and cooperation on trade, he does not disaggregate the influence of various types of cooperative events, such as alliances.

19. Of course, trade barriers and trade flows need not always move in tandem. Tariffs, for example, may not be completely endogenized if demand is infinitely inelastic with respect to price. In addition, the 1970s witnessed the expansion of both trade barriers and trade flows. However, once we control for other determinants of trade, we would expect these variables to be directly related; and this approach should therefore provide a useful first cut at the problem.

20. For the period after World War II, we analyze West Germany; for the period before 1917, Russia. In order to simplify the presentation, however, we refer only to "Germany" and "the Soviet Union" in the text.

21. Small countries are excluded from this study, in addition, because they would not ordinarily be expected to be able to influence their terms of trade.

22. For the period from 1938 to 1985, all data on exports are taken from the International Monetary Fund's *Direction of Trade* for the years 1950, 1956, 1966, 1976, and 1986. For the period from 1905 to 1930, data are taken from Cacciapuoti 1928, 87–90; Clarke and Matko 1983, 62–73; France, Bureau de la Statistique Generale 1923, 236; League of Nations 1926, vol. 1, 218–39 and 1933, 328–48; Liesner 1989; Mitchell 1980, 1982, 1983; U.S. Bureau of the Census 1915, 330–37, 355–56; and U.S. Department of Commerce 1934—and also from the Commerce Department's *Foreign Commerce and Navigation of the United States* for 1914, 1921, and 1931 (pp. 7–8, 5–12, and 6–14, respectively) and the Japanese Finance Department's *Annual Return of the Foreign Trade of the Empire of Japan* for the years 1905 and 1920. In those cases where exports are not denominated in U.S. dollars, exchange rates are taken from Bidwell 1970; Liesner 1989, 54–55; and the Treasury Department's *Circulars* for the years 1904–5, 1912–13, and 1919–20.

It should also be pointed out that in some cases, exports by state i to state j are not available. However, in these cases, it is usually possible to find data on imports by j from i. Since these values should be equal, we convert imports in j's currency into U.S. dollars and use these data as measures of exports from i to j. When we do so, we should adjust the data to reflect the fact that the value of imports reported sometimes includes transportation costs. We do not do so, however, because our sources do not uniformly report on whether or not import data are net of transportation costs.

23. Data on nominal GNP for the period from 1904 to 1929 are taken from Gregory 1982; Liesner 1989; and Mitchell 1980, 1982, 1983. In certain cases, reliable data on Soviet and French GNP are not available. In these cases, GNP is estimated using data in Bairoch 1976; Gregory 1982; and Mitchell 1980. Data on nominal GNP and exchange rates for the period from 1937 to 1984 are taken from the United Nations' *Yearbook of National Account Statistics* for the years 1948, 1955, 1965, 1975, and 1985. For exchange rate data covering the period prior to 1937, see n. 22.

24. All data on national population are taken from Correlates of War Project 1991. It should be noted that since no data on West German population are available for 1954, we use data for 1955.

25. Data on geographical distances are found in Fitzpatrick and Modlin 1986 and the U.S. Hydrographic Office's *Table of Distances in Nautical and Statute Miles* for 1912 and 1936 and *Distance between Ports* for 1965 and 1989. For each pair of states, the shortest distance between ports or rail centers was used to measure the distance between states i and j.

26. Throughout this study, Small and Singer's (1969) data on alliances are used. We consider any defense pact, neutrality or nonaggression pact, or entente that is listed by Small and Singer to be an alliance. Because the purpose of Small and Singer's study was to explain war, they excluded all wartime alliances from their data. As they themselves observed, however, if their data are used for other purposes, the list of alliances should be amended correspondingly (p. 262). Thus, we also include as allies any states that fought on the same side of any interstate war, as listed by either Levy (1983) or Small and Singer (1982). For present purposes, this results in the inclusion of one multilateral alliance among Great Britain, France, Japan, and the United States in 1919, when these states participated in the Russian Civil War (Levy 1983). We also code the Japanese-United States Security Treaty (1951–60) and the subsequent Treaty of Mutual Cooperation and Security between these states (1960–present) as alliances. Among other things, these treaties called for the United States to come to the aid of Japan in the event that it was attacked (see Grenville 1974, 270, 286–87). As a result, they served much the same purpose as an alliance.

27. In this study, any war that is listed by either Levy (1983) or Small and Singer (1982) is analyzed. It might be expected that wars would exert a contemporaneous, as well as a lagged, effect on trade. Since both of the wars considered in this study were being waged in year t, as well as year t − 1, any contemporaneous effect these wars might have on trade will be captured by our model. Further, it should be noted that only one war (the Changkufeng War, 1938) would be added to our list of wars if we measured this variable in year t, rather than year t − 1.

28. While this strategy avoids potential problems of simultaneity, it is, of course, possible that the relationship between trade and alliances is multidirectional. Although it would also be useful to assess the effect of trade on alliance patterns, developing an adequate model of alliances is clearly beyond our scope here.

29. Outliers were identified based on the value of studentized residuals (see Fox 1991, 25–29; Maddala 1988, 412–17). Space limitations preclude us from presenting the results here both with and without outliers. The complete set of results for all the analyses in this paper can be found in Gowa (n.d.).

30. In regression analyses of this sort, it is important to ensure that the errors are not heteroscedastic. The results of White tests yielded no evidence of heteroscedasticity for any of estimates reported in this study. See Maddala 1988, 162–63; White 1980.

31. Assuming that this condition obtains, the standard error of the mean ($s(\bar{x})$) of the regression coefficients of log $BA_{ij}$ is estimated by

$$s(\bar{x}) = \sqrt{\frac{\sum_{i=1}^{n} s_i^2}{n^2}},$$

where $s_i^2$ is the estimated variance of the $i^{\text{th}}$ regression coefficient of log $BA_{ij}$ and n is the number of cross sections for which this regression coefficient is estimated.

32. See n. 31.

33. In antilogarithmic form, equation 1 can be expressed as

$$X_{ij(t)} = AY_{i(t-1)}^{B1}\, Y_{j(t-1)}^{B2}\, P_{i(t-1)}^{B3}\, P_{j(t-1)}^{B4}\, D_{ij(t-1)}^{B5}$$
$$BA_{ij(t-1)}^{B6}\, MA_{ij(t-1)}^{B7}\, War_{ij(t-1)}^{B8}\, z_{ij},$$

where $BA_{ij(t-1)}$ is a dummy variable that equals e (the base of the natural logarithms) if a bilateral alliance exists between states i and j in year $t - 1$, and 1 otherwise. It is clear that if $BA_{ij(t-1)} = 1$ (and $\log BA_{ij(t-1)} = 0$), then

$$X_{ij(t)} = AY_{i(t-1)}^{B1}\, Y_{j(t-1)}^{B2}\, P_{i(t-1)}^{B3}\, P_{j(t-1)}^{B4}\, D_{ij(t-1)}^{B5}$$
$$1^{B6}\, MA_{ij(t-1)}^{B7}\, War_{ij(t-1)}^{B8}\, z_{ij}.$$

On the other hand, if $BA_{ij(t-1)} = e$ (and $\log BA_{ij(t-1)} = 1$), then

$$X_{ij(t)} = AY_{i(t-1)}^{B1}\, Y_{j(t-1)}^{B2}\, P_{i(t-1)}^{B3}\, P_{j(t-1)}^{B4}\, D_{ij(t-1)}^{B5}$$
$$e^{B6}\, MA_{ij(t-1)}^{B7}\, War_{ij(t-1)}^{B8}\, z_{ij}.$$

As a result, the proportional increase in the predicted value of exports by state i if there exists a bilateral alliance between states i and j (rather than no bilateral alliance between them) is equal to $e^{B6}$ minus one. Since the mean of $B_6$ in Table 1 is about 1.09 and the standard error of the mean is about .15, if the mean was two standard errors less than 1.09, it would equal .79. And since $e^{.79} = 2.20$, the average effect of the change from the absence of a bilateral alliance between states i and j to the existence of such an alliance is to increase the predicted value of exports from i to j by 120%, when the remaining variables in the model are held constant.

34. See n. 33. This is the case, since $e^{.29} = 1.34$.

35. See n. 29.

36. For two reasons, we do not examine the effects of the GATT in 1955: (1) all of the states in our sample that were GATT members in 1954 (year $t - 1$) were also NATO members; and (2) for balance-of-payments reasons, several European members of the GATT had imposed quotas on imports from the dollar area.

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# UNDERSTANDING THE *CHALLENGER* DISASTER: ORGANIZATIONAL STRUCTURE AND THE DESIGN OF RELIABLE SYSTEMS

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*The destruction of the space shuttle* Challenger *was a tremendous blow to American space policy. To what extent was this loss the result of organizational factors at the National Aeronautics and Space Administration? To discuss this question analytically, we need a theory of organizational reliability and agency behavior. Martin Landau's work on redundancy and administrative performance provides a good starting point for such an effort. Expanding on Landau's work, I formulate a more comprehensive theory of organizational reliability that incorporates both type I and type II errors. These principles are then applied in a study of NASA and its administrative behavior before and after the* Challenger *accident.*

On 28 January 1986, the entire nation focused on a single event. Seventy-three seconds after lift-off, the space shuttle *Challenger* was destroyed in a powerful explosion fifty thousand feet above the Kennedy Space Center. The losses resulting from this catastrophe were quite high. Seven astronauts, including Teacher-in-Space Christa McAuliffe, were killed as the shuttle broke apart and fell into the sea. The shuttle itself had to be replaced at a cost of over two billion dollars. The launch of many important commercial and military satellites had to be delayed as American space policy ground to a complete halt. The accident had a profound impact on the National Aeronautics and Space Administration (NASA), as well. The agency's credibility and its reputation for flawless execution of complex technological tasks were lost, along with the *Challenger*. To this day, the legacy of the *Challenger* haunts the decisions of both the agency and its political superiors in Congress and the White House.

An examination of the shuttle remnants and other launch data revealed the technical cause of the accident. The shuttle was destroyed when an O-ring seal on the right solid rocket motor failed, allowing the escaping hot gases to burn through and ignite the main fuel tank of liquid hydrogen and liquid oxygen. Although identifying the technical cause of this disaster is important, it represents only one aspect of the problem. Perrow (1984) has argued that organizational and technological failures have become so intimately linked that to fully understand the cause of most major accidents, we must analyze both the administrative and technical aspects of the situation. While there have been some administrative critiques of NASA in the wake of this disaster, almost all have centered on issues of bureaucratic culture, such as the agency's propensity to ignore key evidence and its myopic view of its mission. Surprisingly little has been done on a systematic analysis of the NASA organization structure and how it may or may not have contributed to the loss of the *Challenger*.

One reason for this void is that the analytical tool necessary—a comprehensive theory of organizational reliability—is still lacking. Those involved in the debate over structural design have adopted two opposing stances. The traditional public administration focus on organizational design has involved the pursuit of efficiency in the sense of minimizing costs for a given level of output. Implicit in this traditional analysis is that the reliable performance of the organization was constant. The critical question, therefore, has usually been how one might achieve same level of services at a lower cost. As a result, the policy recommendations from this traditional line of thinking have been to streamline administrative systems and reduce organizational redundancy as much as possible.

The work of Martin Landau (1969) on redundancy in organizations was a particularly important contribution to this debate, inasmuch as he recognized that administrative reliability is dependent on structural factors. In breaking with conventional wisdom, Landau's landmark 1969 essay, "Redundancy, Rationality, and the Problem of Duplication and Overlap," asserted that the critical question was not how to cut the costs of administrative performance but, rather, how to ensure the organization's effectiveness. Landau began by noting that, "no matter how much a part is perfected, there is always the chance that it will fail" (p. 350). As he observed, a streamlined system requires only one part to fail for the entire system to fail. Drawing on concepts from engineering reliability theory, Landau argued that redundancy built into the system can make an organization more reliable than any of its parts. Therefore, Landau concluded, administrative redundancy and duplication can be an important part of effective government.

Some work has been done to extend Landau's initial insights regarding organizational redundancy and administrative reliability, primarily by students and colleagues of Landau. Jon Bendor's (1985) *Parallel Systems*, originally written as a dissertation under Landau, was the most comprehensive effort to follow up on these ideas. Bendor formalized many of Landau's concepts and was the first to conduct empirical testing of these propositions, focusing on transportation planning and operations in three American metropolitan areas. Donald Chisholm's (1989) *Coordination without Hierarchy*, also written originally as a dissertation under Landau, discussed the notion of reliability and redundancy as it existed in informal organizational structures. Others have extended some of Landau's concepts of reliability to the operations of air traffic controllers and aircraft carrier battle groups (LaPorte and Consolini 1991; Rochlin, LaPorte, and Roberts 1987).

The policy prescriptions that follow from these two positions are thus quite different. The traditionalist argument tells us to reduce redundancy and streamline administrative systems whenever possible, whereas Landau advocates increasing redundancy by adding parallel units to the system. Most interesting, however, is the fact that an analysis of the organizational changes at NASA reveals that neither the traditionalists nor Landau are fully correct. Certain parts of the space agency followed a traditionalist policy of streamlining, while other segments of NASA adopted the Landau perspective by generating new parallel linkages. Yet (as I shall show), both these decisions were wrong and contributed to the untimely destruction of the *Challenger*.

How can this be? The reason is a fundamental limitation in the current framework for discussing organizational reliability. Most work in this area has implicitly assumed that there was only one kind of institutional failure and thus only two possible states for organizational performance: the agency either adopted the proper policy or not. But it should be recognized that the latter possibility conceals two different problems: the agency can simply fail to act, or it can adopt an *improper* policy. Considering the impact of both forms of error is important, because it leads us to a different set of policy prescriptions. An organizational structure that is effective at preventing one type of error may not be equally effective at preventing the other type of error.

To date, Bendor alone has formally recognized this distinction; but his analysis was limited (1985, 49–52). I shall extend our understanding of organizational reliability by exploring how each kind of failure is affected by different kinds of administrative structures. I shall lay a foundation for an analysis of multiple forms of administrative failure by describing the principles from engineering reliability theory, then show how different kinds of structures leave the agency vulnerable to different kinds of errors. Applying these arguments to the structure and decision-making process of NASA in the pre- and post-*Challenger* eras, I shall argue that the disaster had its roots in the structural changes adopted by the space agency in the 1970s and 1980s.

Although occasionally cited by students of public administration, Landau's work on institutional performance and reliability has received little attention from political scientists. One possible reason for this stems from the engineering roots of reliability theory. Political scientists may have considered the issue of organizational redundancy and reliability simply to be a technocratic problem that could be solved by reference to engineering formulas that dictate the appropriate structural form and do not raise political issues. Indeed, if we limit the scope of the problem to two-state devices, this apolitical view of structural design has some merit. But in a system with multiple types of errors, trade-offs between the errors must be made; and this moves us into the realm of politics. Which goals will be embraced? How will resources be allocated to combat each kind of error? Answers to these questions are ultimately political issues.

## TWO TYPES OF ADMINISTRATIVE ERRORS

Before proceeding further, it is important to discuss more thoroughly what is meant by the term *policy failure*. As just noted, administrative problems often have a richer structure than the simple two-state (operating/failed) model can incorporate. In particular, bureaucracies are often in the position to commit two types of errors; (1) implemention of the wrong policy, an error of commission; and (2) failure to act when action is warranted, an error of omission. If we consider the agency's decision to take action to be comparable to the acceptance of a hypothesis, we can relate these two kinds of failures to the more familiar type I and type II errors often studied in statistics.

To establish this link, we must first define the null and alternative hypothesis in terms of potential bureaucratic action. Since presumption often favors the status quo, let us consider the null hypothesis to be that the agency should not take any new action and the alternative hypothesis to be that the bureau should take such action. Therefore, if the agency chooses to act when it is improper to do so (rejects the null hypothesis when it is true), a type I error has been committed. Likewise, if the bureau fails to act in a situation where it should (accepts the null hypothesis when it is false), then a type II error has been committed.

To illustrate the concept of type I and type II errors in organizational systems, let us consider the example of NASA and its decision to launch the space shuttle. The space agency traditionally approaches the launch decision with the assumption that a mission is not safe to fly. Subordinates are then required to prove that such is not the case before the launch is permitted. The null hypothesis, therefore, is that the mission should be aborted. If NASA were to reject the null hypothesis by launching a mission that is actually unsafe, it would be committing a type I error. On the other hand, if NASA decided not to launch a mission that was technologically sound, then it would have committed a type II error. The agency's choices and consequences are summarized in Figure 1.

Each form of failure is associated with a different set of costs. By committing a type II error, NASA loses the opportunity to achieve its objectives and wastes time, effort, and materials that could have

FIGURE 1

**Summary of NASA Responses and Possible Errors Regarding Launch Decisions**

| | | The proper course of action: | |
|---|---|---|---|
| | | Launch | Abort |
| NASA decides to: | Launch | Correct Decision<br>Mission successful | Type I Error<br>Accident occurs; Possible loss of life and/or equipment |
| | Abort | Type II Error<br>Missed opportunity; wasted resources | Correct Decision<br>Accident avoided |

null hypothesis: the mission should be aborted.

been usefully employed elsewhere. For example, the shuttle's propellants in the external tank, liquid hydrogen and liquid oxygen, are lost if the mission is scrubbed and have alone been valued at approximately five hundred thousand dollars. Furthermore, the agency may forfeit the opportunity to carry out rare scientific research, as was the case when NASA missed the launch date for its ASTRO mission to study Halley's Comet. The *Challenger* accident clearly demonstrated, however, that a type I failure may be far more costly. The destroyed shuttle was replaced by the *Endeavour* at an expense over two billion dollars, and the death of the seven astronauts represents an incalculable loss. Certainly, in the case of NASA, type I errors are associated with greater costs than type II failures. The exact cost trade-off between these types of failure would vary, of course, for different agencies and according to the individual circumstances prevailing at the time of each decision. For this reason, it is important to develop a general approach to the study of organizational reliability that recognizes both types of errors and identifies the consequences associated with them.

By recognizing both forms of potential failure, we are better able to understand the nature of the trade-offs demanded. As with hypothesis testing, gains in type I reliability often come at the expense of type II reliability. However, just as it is conceivable to reduce both $\alpha$ and $\beta$ in hypothesis testing by increasing the sample size, it is possible to increase both type I and type II reliability by raising an agency's resource levels. But because of resource limitations in the real world, it is inevitable that bureaucrats and their political superiors will have to strike a balance between each type of reliability. On the whole, three-state devices allow us to consider both forms of error and thus provide a richer framework for studying the issue of organizational reliability.[1]

## BASIC CONCEPTS OF STRUCTURE AND RELIABILITY

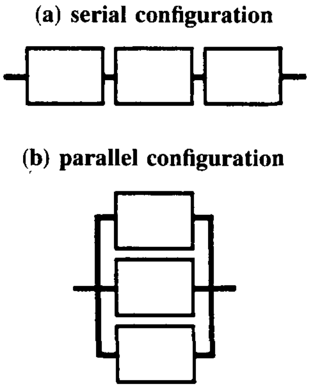
### General Assumptions

Throughout this discussion I will be contrasting *component* and *system* reliability. It is important to clarify, in advance, what is meant by these terms. A system is a collection of subunits, known as components, which are linked together in a particular structure. In administrative theory, identifying components is dependent on the system level in question. If one were concentrating on the behavior of an agency, then the agency as a whole is the system, and the offices within it are the components. Alternatively, if one were looking at the executive branch as a whole, that would be the system and each agency a component within the system. In an organizational context, determining what the components are depends largely on the system with which you are concerned. In this case study of NASA, the system of concern is the agency, and each component represents an office or division inside NASA.

Three other assumptions of this work must also be specified. First, I start by assuming that the probability of failure for each component in the system has already been determined either by testing or past history,[2] then, later, relax this assumption, demonstrating that the theoretical framework is valuable even when the exact probabilities of component failure are not known. Making this assumption at the onset, however, is useful for developing the theory; since the components are assumed to be known quantities, the remaining question is how to assemble these components into a reliable network. Second, I assume that organizational reliability is static, not dynamic. In the engineering literature, reliability is often treated as time-dependent so as to simulate the breakdown of mechanical components; the probability of such failure naturally increases with age. For administrative systems, however, it is not clear how component reliability would change over time. One might argue that agents become more reliable over time because they have greater experience and expertise with the issues and are thus better able to address them. On the other hand, it could be said that agents are less reliable over time because they are more secure in their positions and lose their incentive to perform well. Additionally, interest-group capture of some public agency may affect the reliability of its performance. While these ideas raise interesting questions, static models of administrative reliability will provide sufficient insight for our needs here. Finally, I assume that the states of all components are statistically independent. In other words, the failure of one component or subsystem does not affect the probability of failure of other components. Those who study public administration may question the

FIGURE 2

**Redundant System in Parallel**



validity of this assumption. Bendor, however, proves a theorem demonstrating that some degree of component interaction does not negate the general results of reliability theory (1985, 44–49). Therefore, the assumption of component independence is a useful simplifying assumption that does not undercut the generalizability of the results. With these assumptions in hand, we can now focus our attention on the structural configurations commonly found in organizations.

### Types of Organizational Structures

There are several basic organizational forms employed in the development of administrative systems. The first is a serial structure, a form often found in organizations. In a traditional serial structure, pictured in Figure 2, the first component must correctly process a policy initiative before sending it on to the next component. To be effective, policy must successfully pass through each of these components. The result is that in order for the system as a whole to fail, it is only necessary for one of the components in series to fail. If any one component were to fail to pass the policy to the next unit, then all the components that followed it would be unable to act, and the policy could not get through the system.

Another possible organizational form is the establishment of parallel linkages between components. In a parallel structure, such as the one illustrated in Figure 2, a policy may pass through any one of the components in order to get to the implementation stage. It is different from the serial structure inasmuch as even if one or more units fail to pass the policy along, it may still be able to make it through the system. The end result is that for a policy to fail to get through this type of system, all components must fail.

One variation of this structural form is the k-out-of-m unit network. In certain cases, we require that a certain number, k, of the m units in an active parallel redundant system must work for the system to be successful. One example of this type of system might be the requirement that at least two of the space shuttle's four major computers be on-line for launch. An organizational application of this system would be an agency director's decision rule not to implement any policy that a majority of the staff cannot agree upon ($k = \frac{m}{2} + 1$).

There are two special cases of the k-out-of-m unit network which merit attention. The first instance is where k = 1, in which case we are back to a simple parallel system. The second case is where k = m, which effectively reduces the structure to a serial system. (See Appendix for proof.) In the latter case, although the network is configured as an active parallel system, in terms of reliability it is behaving as a serial structure. To differentiate between this type of system and a traditional serial network, we will classify this type of structure as a *serially independent* system. This name signifies that while the system is essentially a serial one, its components operate completely independently from each other.

In an organizational context, an important distinction between a serially independent system and the traditional serial structure is the difference in processing time. In a traditional serial system, the first component must process the information, then pass it on to the next component for processing. This continues until all the components have processed the information. The total processing time of the system is the sum of processing times for each component plus some factor to account for transmission delay between units. In contrast, the serially independent system allows all components to operate simultaneously. The time needed for the serially independent system to complete its task is simply the time it takes the slowest component to finish its operations. So while both structures yield the same level of reliability, serially independent systems require less processing time.

Larger organizations often utilize combinations of serial and parallel structural forms. Two examples of this can be seen in Figure 3. In a series–parallel system there are several parallel subsystems linked together in a serial fashion. A parallel–series configuration is the result of several serial substructures combined into a larger parallel network. As these two examples illustrate, most large and complex structures can be decomposed into smaller units for easier analysis.

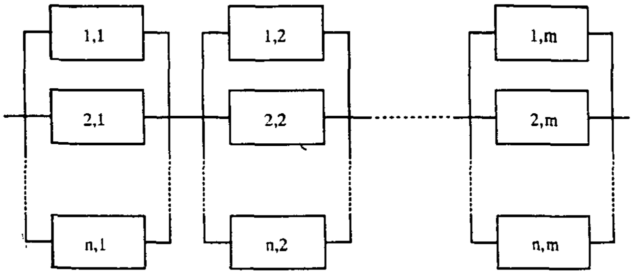
## THE ADVANTAGE OF PARALLEL SYSTEMS IN A TWO-STATE WORLD

Components aligned in a series configuration are perhaps the easiest systems to analyze, as well as the most commonly encountered. As I noted earlier, in order for the system as a whole to fail, only one of the components in series has to fail. Defining the probability of failure for component i as $f_i$, a mathematical statement of the reliability of a serial system with m components would be

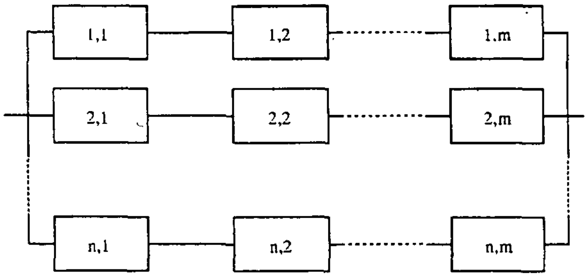
$$R_{sys} = \prod_{i=1}^{m} (1-f_i). \tag{1}$$

**FIGURE 3**

**Combinations of Structures with m Units in Series and n Units in Parallel**



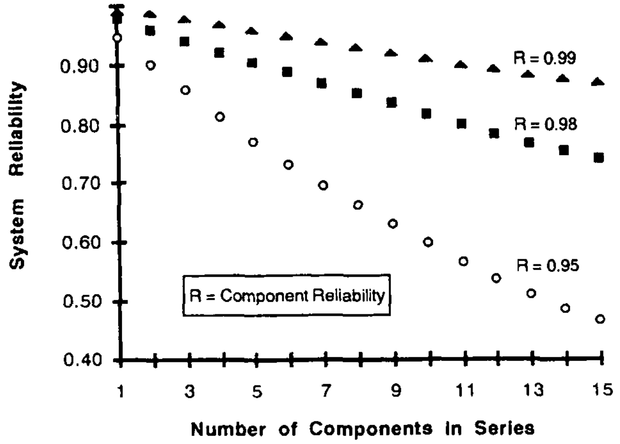
(a) Series-Parallel Configuration



(b) Parallel-Series Configuration

**FIGURE 4**

**Declining Reliability of Serial Systems**



We can see that two factors determine the reliability of a series system: component reliability and the number of components in the system. In order to increase the reliability of this type of system, one must either increase the reliability of the components or decrease the total number of components employed. As illustrated in Figure 4, marginal gains in system reliability from increasing component performance decrease as component reliability increases. Because the costs of increasing component reliability often rise exponentially, it is more effective in many cases to reduce the total number of components in series to reach reliability goals.

Perhaps the most common means of increasing reliability in a two-state world is to add parallel components to a system. It is assumed that all branches are active and that a signal needs to pass through only one branch to be successfully transmitted. Since it is assumed that all branches must fail in order for the system to fail, the reliability function for a parallel system with n components is simply

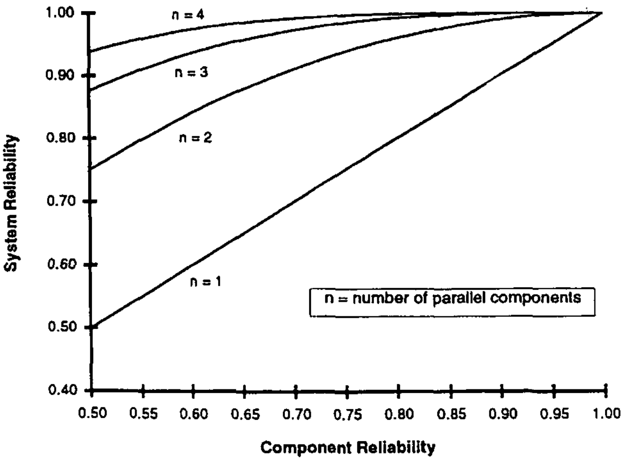
$$R_{sys} = 1 - \prod_{i=1}^{n} f_i.$$

Figure 7 illustrates the relationship between component reliability, the number of parallel elements, and overall system reliability. In this case, we see that the marginal gains from adding parallel channels decrease as the number of parallel components increases.

In comparing Figures 4 and 5, the attractiveness of parallel systems in a two-state world is clear. Holding component reliability constant, the addition of redundancy in a parallel fashion will raise the reliability of the overall system, while creating serial redundancies decreases total system reliability. It is not surprising, therefore, that many scholars in this area have spurned serial systems and focused, instead, on parallel linkages when discussing this issue. Pressman and Wildavsky (1973) have criticized the existence of "multiple clearance points" (a serial system) for an implementive decision because it reduces the likelihood that any policy can ever be executed. Landau (1969) directs the same logic against "streamlined" serial structures, claiming that such systems are more susceptible to failure than those with paral-

**FIGURE 5**

**Increasing Reliability with Parallel Systems**



lel redundancies. As a result, the major policy prescription of the work of Landau (1969, 1973, 1991) and others (Bendor 1985; Lerner 1986) has been to emphasize the use of parallel linkages in order to create more reliable organizations.

However, if it is true that serial systems are generally less reliable, why are such structures ever adopted by organizations? To answer this question fully, we must go beyond the two-state world to allow for multiple forms of error.

## TYPE I AND TYPE II ERRORS IN AN ADMINISTRATIVE SETTING

### The Advantages of Serial Systems in a Three-State World

As we have seen in the two-state world, serial structures are less reliable than others of comparable size. Nonetheless, it is clear that this structural form has been employed repeatedly in the development of bureaucracies. The reason this type of organizational structure has survived, I will suggest, is that it is valuable when we must design systems to accommodate multiple types of error. I previously assumed that there existed only two states for every component: good (operating) and failed (not operating). I shall now elaborate a theory of organizational reliability that allows for the existence of both type I and type II errors.

In general, a series structure is better suited to stop type I errors from occurring. Since the null hypothesis is that the agency should not take any action, if any component chooses to pass the proposed policy through its part of the system, then it is rejecting the null hypothesis. For any policy to pass through a serial system successfully, all units must agree to pass it along. If rejecting the null hypothesis was actually the incorrect decision (a type I error), then all units must commit such an error for the serial system as a whole to fail. Mathematically, we can identify both the probability of a type I failure occurring in the system ($F_\alpha$) and the reliability of the system against this error ($R_\alpha$) as

$$F_\alpha = \prod_{i=1}^{m} \alpha_i$$

$$R_\alpha = 1 - \prod_{i=1}^{m} \alpha_i,$$

where $\alpha_i$ is the probability of a type I error occurring in the i$^{th}$ component and m is the number of components linked in series.

For an example, consider NASA's launch decision process. We would find that a serial structure is more effective in preventing unsafe launches. In order to approve an unsafe launch in a serial system, every unit must err. The more hurdles are established (via increasing numbers of serial components), the harder it is for an unsafe launch proposal to pass through the system unopposed.

With regard to type II errors, however, series structures are less effective. Consider the fact that if one unit accepts the null hypothesis of no new action, then the policy cannot be passed through the rest of the serial system. If accepting the null hypothesis was actually incorrect (a type II error), then the whole system would have failed. Thus, in order for a type II error to occur at the system level, only one component in series must fail in this manner for the system to fail. The more components added in series, the greater the probability that a component will commit a type II error, causing the system to fail ($F_\beta$). The reliability of a series structure with regards to type II errors ($R_\beta$) can be represented as

$$F_\beta = 1 - \prod_{i=1}^{m} (1 - \beta_i)$$

$$R_\beta = \prod_{i=1}^{m} (1 - \beta_i),$$

where $\beta_i$ is the probability of a type II error occurring in the i$^{th}$ component and m is the number of components linked in series.

We may also be interested in the overall reliability of the system—the likelihood that an agency would commit either a type I or type II error. To find this, we assume that for a given event at a given time, it is not possible for an administrative system to commit both a type I and a type II error simultaneously. This assumption is not unreasonable. A type I error requires that the agency act, while a type II error demands that the agency postpone any action. An organization cannot both act and not act at the same time. Consider NASA's decision to launch the shuttle. The space agency can either decide to launch the shuttle now (possibly resulting in a type I error) or abort the launch until another time (possibly resulting in a type II error). It cannot decide to both launch and abort at the same time.

Given this argument, we can conclude that type I and type II errors are, at any point in time, mutually exclusive events. Therefore, the probability of either a type I or type II error in a serial system can be found as

$$P(F_\alpha \cup F_\beta) = P(F_\alpha) + P(F_\beta) = \prod_{i=1}^{m} \alpha_i + \left(1 - \prod_{i=1}^{m} (1 - \beta_i)\right).$$

The overall reliability of the serial system is then found to be

$$R_{sys} = 1 - P(F_\alpha \cup F_\beta) = 1 - \left(\prod_{i=1}^{m} \alpha_i + 1 - \prod_{i=1}^{m} (1 - \beta_i)\right)$$

$$R_{sys} = \prod_{i=1}^{m}(1-\beta_i) - \prod_{i=1}^{m}\alpha_i.$$

A brief numerical example will demonstrate these properties of serial systems. Assume that the NASA launch decision structure has two components that operate in series, each having the probability of committing a type I error $\alpha = .10$ and the probability of committing a type II error $\beta = .20$. The probability that the overall system approves the unsafe launch (type I) is $F_\alpha = .01$, which is much less than either of the two components. However, the probability that NASA will abort a safe launch (type II) is $F_\beta = .36$. The system as a whole is more likely to commit the type II error than either of its elements. The overall reliability of the system is found to be $R_{sys} = .63$.

If we concern ourselves only with the system's overall performance, we can find the number of elements in series that, for a given level of component reliability, will result in the optimal level of reliability. The system reliability of a series network consisting of m identical and independent components is $R_{sys} = (1 - \beta)^m - \alpha^m$. Differentiating this equation with respect to m and setting the result equal to zero will give us the optimal number of components to be linked in series in order to maximize the system's reliability. This result (derived in the Appendix) is

$$m^* = \frac{ln\left(\dfrac{ln\ \alpha}{ln(1-\beta)}\right)}{\left(ln\ \dfrac{(1-\beta)}{\alpha}\right)}.$$

## The Disadvantage of Parallel Systems in a Three-State World

As the reader will have realized by now, there is a inverse relationship between the reliabilities of series and parallel systems. For instance, although series structures are ineffective against type II errors, parallel systems are able to reduce the probability of such errors occurring. This is because it is necessary for all components to fail in this manner for the overall system to commit a type II error. Likewise, type I errors are increased in such a framework, because it is only necessary for the incorrect action to pass through one channel in order to be implemented by the system. Therefore, the more independent parallel branches are attached to a structure, the more likely a type I error will occur but the less likely a type II error will be committed by the system.

The mathematical formulation of the reliability of a parallel system is

$$F_\alpha = 1 - \prod_{i=1}^{n}(1-\alpha_i)$$

$$R_\alpha = \prod_{i=1}^{n}(1-\alpha_i)$$

$$F_\beta = \prod_{i=1}^{n}\beta_i$$

$$R_\beta = 1 - \prod_{i=1}^{n}\beta_i$$

$$R_{sys} = \prod_{i=1}^{n}(1-\alpha_i) - \prod_{i=1}^{n}\beta_i.$$

Modifying the example used earlier, let us assume that instead of connecting the two units in series, NASA links its components together in a parallel network. In this case, launch will occur if either unit recommends it. Now, the probability of launching an unsafe mission is greater, $F_\alpha = .19$; but the probability of aborting a good mission is only $F_\beta = .04$. The overall reliability of this system in preventing either type of error is much greater than before: $R_{sys} = .77$ in parallel, while $R_{sys} = .63$ in series.

Just as we were able to do earlier for the series structure, we can calculate the optimal number of identical and independent components linked in parallel. The system reliability of a parallel network consisting of n identical and independent components is $R_{sys} = (1 - \alpha)^n - \beta^n$. Differentiating this equation with respect to n, and setting the result equal to zero will give us the optimal number of components to be linked in parallel in order to maximize the system's reliability. This result (derived in the Appendix) is as follows:

$$n^* = \frac{ln\left(\dfrac{ln\ \beta}{ln(1-\alpha)}\right)}{\left(ln\ \dfrac{(1-\alpha)}{\beta}\right)}.$$

## Combinations of Serial and Parallel Systems

In hypothesis testing, the probability of type I and type II errors can be simultaneously reduced by increasing the sample size. Similarly, we can lower the probability of both types of error occurring in reliability theory by adding additional components to the system both in series and in parallel.

With regard to multiple errors, we may employ the expressions derived earlier to analyze the reliability of networks that contain both parallel and series subsystems. To do this, we must first reduce the overall structure into a set of serial and/or parallel subsystems. The various subsystems are evaluated to find the probability of type I and type II failure at this level. Each subsystem is then treated as a single component in a larger model of the overall structure.

Using this system reduction method, we are able to find the reliability of more complex administrative networks.

Finding the optimal number of components in a mixed structure is more difficult than it was for simple series or parallel systems. Consider a combined system having m components in series and n components in parallel. We can raise the overall reliability level of the system to a point arbitrarily close to 1 by simply increasing the number of components in both series and parallel without bound (Barlow and Proschan 1965, 187). However, given a fixed number of components in either series or parallel, we can find the optimal number of components needed in the other dimension. I shall discuss this for both series–parallel and parallel–series systems.

First, let us consider a series–parallel system having m components in series and n components in parallel (as in Figure 4). All components are assumed to be identical and independent. Regarding the system only as a series, we find that its overall reliability is

$$R_{sys\text{-}sp} = (1 - F_\beta)^m - F_\alpha^m.$$

Next, we must find the probability of type I and type II errors in each parallel subsystem.

$$F_\alpha = 1 - (1 - \alpha)^n$$

$$F_\beta = \beta^n.$$

Combining these equations, we find that the overall system reliability of the n × m series–parallel network is

$$R_{sys - sp} = (1 - \beta^n)^m - (1 - (1 - \alpha)^n)^m.$$

Once we have the equation for the overall reliability of the system and have decided whether to fix the level of m or n, we can find the optimal number of components in the other dimension by differentiating $R_{sys\text{-}sp}$ with respect to the variable we seek to optimize and setting the result equal to zero. There are a number of computer routines available that are also capable of solving this type of problem.

This approach also works for parallel–series systems. Figure 5 shows a parallel–series network of n × m independent and identical components. Using the same method as before, we find the overall system reliability of a parallel–series system to be

$$R_{sys - ps} = (1 - \alpha^m)^n - (1 - (1 - \beta)^m)^n.$$

Again, once we have the equation for the overall reliability of the system and have decided whether to fix the level of m or n, we can find the optimal number of components in the other dimension by differentiating $R_{sys\text{-}ps}$ with respect to the variable we seek to optimize and setting the result equal to zero.

## ALTERING RELIABILITY THROUGH SYSTEM LINKAGES AT NASA

This theoretical approach to structural design can now be used to examine the institutional failures at NASA that ultimately led to the destruction of the *Challenger*. I will show that during the 1970s and 1980s, NASA altered its organizational structure in order to achieve different reliability goals. I examine changes within two specific areas of the NASA's structure that the Rogers Commission mentioned in its report on the *Challenger* accident in 1986. The first area of concern involves the organization of NASA's reliability-and-quality-assurance (R&QA) functions. The second area involves changes that took place in the agency's launch decision structure.

### Changes Within NASA's Reliability-and-Quality-Assurance Function

Prior to 1961, NASA had no explicit reliability-and-quality-assurance function within the agency.[3] In trying to match the Soviet space achievements, NASA devoted little of its effort toward preventing bad launches (type I error) and used almost all its resources trying to launch as often as possible (avoiding type II error). As a result, the agency's mission success rate from 1958 to 1961 was dismal (Weiss 1971).

At that point, the Soviet Union seemed to be winning the space race, inasmuch as they were able to draw attention to their many successes, while the United States had experienced a number of visible failures.[4] It became clear that the only hope of beating the Soviets was for the United States to play down the numbers of launches and emphasize mission quality. This fact underscored the need for an agencywide effort to increase type I reliability. Consequently, NASA administrator James Webb established the first R&QA function within the space agency in 1961.

The R&QA function was initially formed on three levels: within headquarters, at the field centers, and in the contractors' plants. As such, it represents a classic example of serial redundancy. For a mistake to be made by NASA, the error would have to pass all through three checkpoints undetected. As I have noted, system reliability with regard to type I error increases with each additional serial component. Constructing such an organizational structure is wholly consistent with the agency's increased concern over type I reliability at this time.

Following the success of the Apollo program in the 1970s, NASA faced less demand for type I reliability and more for type II reliability. In the Apollo era, NASA's primary concern was for successful achievement. The agency had a specific mandate that it knew had to be fulfilled at any cost. Having secured victory in the race to the moon, NASA faced increasingly tighter budget constraints. At the same time, the demand from politicians for services continued un-abated. This meant that NASA now had to do more with fewer resources available. As a result of the changing political and economic landscape, the agency focus in the Shuttle era had shifted to the efficiency and cost-effectiveness of its policies (Heimann 1991).

By demanding that NASA develop more cost-effective policies, politicians put the agency under greater pressure to pursue type II reliability. As noted earlier, type II failure costs are generally associated with wasted resources and inefficient behavior. From a short-term perspective, then, a concern for type II reliability appeared to be more sensible than type I reliability.[5] The greater pressure for efficient space policy, therefore, led NASA to allocate more of its resources to type II reliability.

The results of this shift are visible in the changes in the R&QA structure since 1970. At the headquarters level, there was a consolidation of the R&QA Office with the Safety Office to take advantage of economies of scale.[6] Later in 1973, this office was combined with other staff and placed under the associate administrator for organization and management.[7] Some of the R&QA work was integrated with the Office of Procurement at this time. Further consolidations occurred in 1977, and all safety- and reliability-oriented functions were transferred to the Office of the Chief Engineer.

Placement of the R&QA function within the Office of the Chief Engineer did not promote type I reliability. Documentation from NASA makes it clear that while safety and reliability were considered important, they were a secondary function of the chief engineer's office (National Aeronautics and Space Administration 1983). The efforts of R&QA were also hampered by the continual loss of personnel. From 1970 to 1985, NASA experienced a 31% decline in total personnel; but within the total R&QA function, this decline was over 62%. As a result, the NASA staff allocated to R&QA was just over 5.1% in 1970; in 1985, it was less than 2.8%.[8]

By the end of 1985, the R&QA staff at NASA headquarters totalled only 17 people. As Chief Engineer Silveira stated in an interview, "We were trying to use the field center organizations rather than having that function here."[9] NASA had all but eliminated one of the serial components in its safety and reliability-and-quality-assurance function. This action reduced the probability of NASA's committing a type II error but increased the chances of experiencing a type I failure.

The R&QA function was not immune to changes at the field centers. This level experienced reductions in manpower, as well.[10] From 1970 to 1985, the three major centers for manned space flight (i.e., the Johnson Space Center, Kennedy Space Center, and Marshall Space Flight Center) cut R&QA personnel by 38, 54, and 84%, respectively. The heavy cuts at Marshall, in particular, were understandable, given the pressure the center was under to be cost-effective. Again, cutting R&QA personnel can be seen as reducing the serial linkages at this level that can serve to prevent Type I errors.

In addition to the personnel reduction, the field centers' ability to supervise the activities of contractors was limited in this period. Normally, NASA seeks to "penetrate" its contractors to provide an adequate check on their work. Aerospace contractors, however, generally conduct business with the Department of Defense, as well as NASA. The Defense Department in the 1970s became concerned that too many NASA inspectors at the contractors' plants could jeopardize national security and wanted to place a cap on the number and scope of their inspection activities (Smith 1989, 230–31). Inasmuch as the agency was dependent on the Defense Department for political support for the shuttle program, NASA had little choice but to accept these limitations on the plant inspection efforts.

After the *Challenger*, NASA's R&QA function changed dramatically. The Rogers Commission castigated the space agency for its "silent safety program" and recommended that it revitalize its R&QA function. In response, NASA created the Office of Safety, Reliability, Maintainability, and Quality Assurance (later renamed the Office of Safety and Mission Quality). Established as a level 1 organization, this office is headed by an associate administrator who reports directly to the NASA administrator. Staffing for the new headquarters office was immediately doubled and has since experienced more than 350% growth over its 1985 level. While NASA personnel levels have grown by 10% over the past five years, the R&QA function as a whole has increased by 123% during that time. As a result, R&QA as a percent of total NASA staff is back at 5.6%, similar to its position at the time of Apollo 11 in 1969; NASA has made a strong and visible effort to restore the serial component at this level.

Within this office, there exist several divisions, such as the Safety Division and the Space Station Safety and Product Assurance Division, which formulate office policy in their respective areas and provide some monitoring to ensure compliance. The real "teeth" of the office, however, are found in the Systems Assessment Division and the Programs Assurance Division. In Systems Assessment, top-level senior engineers from a wide range of disciplines perform independent evaluation of technical problem areas and testing of systems readiness. The Programs Assurance Division also employs senior engineers to identify critical technical problems and to ensure that the office's concerns are properly addressed by the program organizations and field centers. Inasmuch as either of these two units have the ability to stop a launch perceived to be unsafe, these two divisions work as serially independent linkages within the headquarters office. Creating two serial components within the larger serial component at headquarters increases the organization's reliability with regard to type I errors.

The R&QA function at the field center level has also been resuscitated in the wake of the *Challenger* failure. Marshall, which had initiated the largest cuts in this area, has since increased its R&QA personnel by 178% over 1985 levels. R&QA staffing levels at the Kennedy Space Center and the Johnson Space Center have also increased over 1985 levels by 175% and 56%, respectively.

The agency has further augmented its R&QA function through the development of the NASA Safety Reporting System (NSRS). Run by a contractor with no other NASA business, the NSRS provides employees with a confidential means of reporting problems that they believe have not been properly addressed. The system acts as an additional serially independent component in the NASA R&QA structure. Officials at the Office of Safety and Mission Quality state that there have been "no showstoppers" among the reports received by the NSRS. Furthermore, the problems coming through the NSRS have almost always been identified by the office first, evidence that their administrative structure is doing its job properly.

It is important to consider these organizational changes in the context of three-state reliability theory. In Table 1 (Set A), I analyze the various R&QA structures first under the assumption that each component has a 15% chance of making type I and type II errors. The original Apollo structure had three serial units, reducing the change of a type I error to a remote .3%. The advantage of the Apollo structure can be clearly seen. Even if the individual components are not that reliable with regard to type I error, the structure as a whole would guard against such a failure. As we noted earlier, the NASA structure in the Shuttle era prior to *Challenger* was effectively reduced to a single unit. This shift resulted in a dramatic reduction of type II errors and lowered the probability of either form of system failure from 38.9% to 30.0%. The disadvantage is that a type I error, such as the *Challenger* accident, would be far more likely, rising in this case from .3% to 15.0%. Since then, NASA has made a concerted effort to restore the serial structure in R&QA and has even added an additional unit through the Office of Safety and Mission Quality. Adding the fourth component to the serial unit in this case lowers the probability of a type I error to a paltry .1%.

Some could argue that a change to a more streamlined structure would be acceptable if accompanied by an increase in component reliability. This is not necessarily the case. In Set B, I allow each component to lower its probability of error by a factor of three, from 15% to 5%. Under these circumstances, the pre-*Challenger* structure has a 5% chance of committing a type I error. The Apollo structure, even with less reliable components, had less than a 1% chance of committing such an error. This comparison makes it clear that these structural changes at NASA were consequential. As this analysis has shown, streamlining the R&QA function increased the probability that a type I failure such as the *Challenger* accident would eventually occur.

**TABLE 1**

**Probabilities of Failure for NASA R&QA Structures (all results expressed as a percentage)**

| COMPONENTS & STRUCTURES | SET A | SET B |
|---|---|---|
| Component Failure | | |
| Type I error | 15.0 | 5.0 |
| Type II error | 15.0 | 5.0 |
| Apollo Structure | | |
| Type I error | 0.3 | <0.1 |
| Type II error | 38.6 | 14.3 |
| Overall error | 38.9 | 14.3 |
| Shuttle Structure | | |
| *Before Challenger* | | |
| Type I error | 15.0 | 5.0 |
| Type II error | 15.0 | 5.0 |
| Overall error | 30.0 | 10.0 |
| *After Challenger* | | |
| Type I error | 0.1 | <0.1 |
| Type II error | 47.8 | 18.5 |
| Overall error | 47.8 | 18.5 |

*Note*: A Type I error for NASA would be a decision to launch an unsafe mission. A Type II error would be a decision to abort a technically sound mission.
The figures for component reliability, as well as the calculations which follow, are not empirically derived estimates but rather they are assumptions made for the purpose of illustration.

### Changes in the Launch Decision Structure

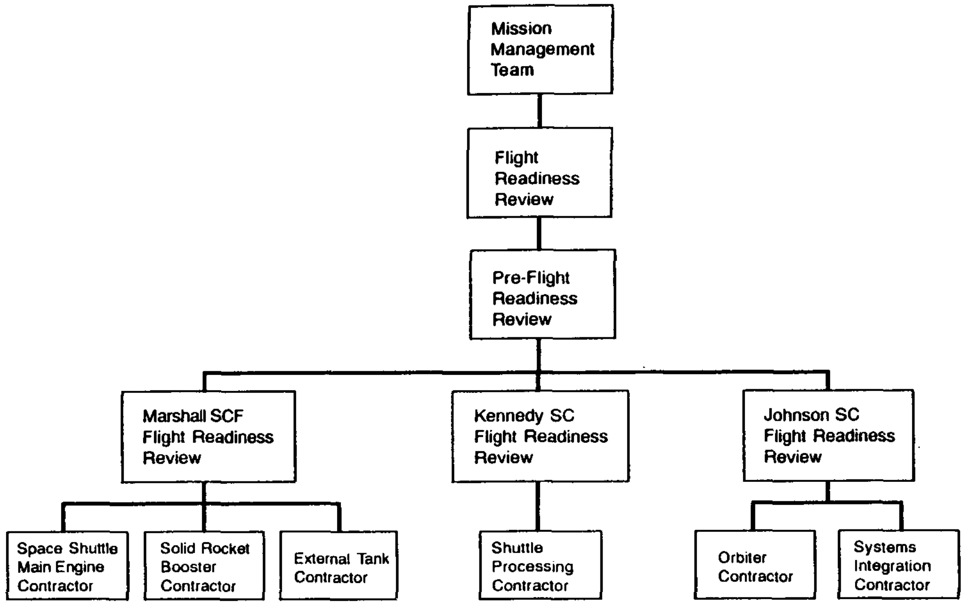
At the same time that the R&QA function at NASA was undergoing significant modifications, important changes in the launch decision process also occurred that exacerbated the problem of type I reliability at NASA. I shall examine both the development and the impact of structural changes in the launch decision process prior to the *Challenger*.

Figure 6 illustrates NASA's official launch decision structure. Designed originally in the Apollo era to limit type I errors, the system has a large number of serial components. Although the field centers are configured as parallel units in the diagram, this level of the structure actually operates as a serially independent system. The reason for this is that the operating rule at the preflight review is that if any center reports it is unready to fly, the mission is aborted. This is an example of a k-out-of-m network, where k = m. While most of this structure remained intact throughout the shuttle program, critical changes occurred at the Marshall Space Flight Center.

Marshall has responsibility for three aspects of the shuttle program; the main engines, the external tank, and the solid rocket boosters. At the center, there is a project manager and staff assigned to each section of the program. Before a launch, contractors for each element in the shuttle must certify in writing that their components have been examined and are ready to fly the specified mission. After this step, the Marshall staff responsible for that segment of the program must also verify that it is safe to fly under

FIGURE 6

**NASA Official Launch Structure, 1981–1986**


Mission Management Team
Flight Readiness Review
Pre-Flight Readiness Review
Marshall SCF Flight Readiness Review
Kennedy SC Flight Readiness Review
Johnson SC Flight Readiness Review
Space Shuttle Main Engine Contractor
Solid Rocket Booster Contractor
External Tank Contractor
Shuttle Processing Contractor
Orbiter Contractor
Systems Integration Contractor

these conditions. This process is illustrated in Figure 7.

This structure works well at preventing type I failures, but it is not as effective with regard to type II errors. Assume for a moment that the probability of each component in this system committing a type I error is 5% and the chance of a type II error is 15%.[11] In such a case, each subsystem (solid rocket boosters, main engine, and external tank) has only a .25% chance of committing a type I failure; but a 27.75% chance of allowing a type II error. The probability that the center as a whole would be responsible for any type II launch failure is 62%.

This is certainly not good news for Marshall, which was under a great deal of pressure to operate cost-effectively. The center had been slated for shutdown following the completion of Apollo. As former NASA administrator James Fletcher told his successor, "Closing Marshall has been on the Office of Management and Budget's agenda ever since I came to NASA in 1971" (Smith 1989, 84). To prevent this from occurring, NASA headquarters sent several high-visibility projects, such as the Space Telescope and the shuttle's solid rocket motors, to Marshall. Despite these efforts, Marshall was still threatened with large reductions in personnel and other resources following Apollo.[12]

This pressure became even more intense as the shuttle neared operational status. In 1978, the OMB had expressed to NASA that it wished to impose major cuts at Marshall. Although NASA suggested "new roles" for the center, OMB officials made it clear they were not interested.[13] While NASA as an institution faced strong pressure to run its operations cost-effectively, Marshall felt this demand more intensely. Consequently, managers at the center sought to increase their type II reliability, so as not have a launch stopped on account of a Marshall part (McConnell 1987, 109, 112).

To achieve this objective, Marshall Center director William Lucas unofficially changed the organizational structure from a serial system to a parallel one (Figure 8). If either the contractor or the Marshall staff stated that a launch was justifiable, Lucas would insist that

FIGURE 7

**Official Structure of Flight Readiness Review at the Marshall Space Flight Center**

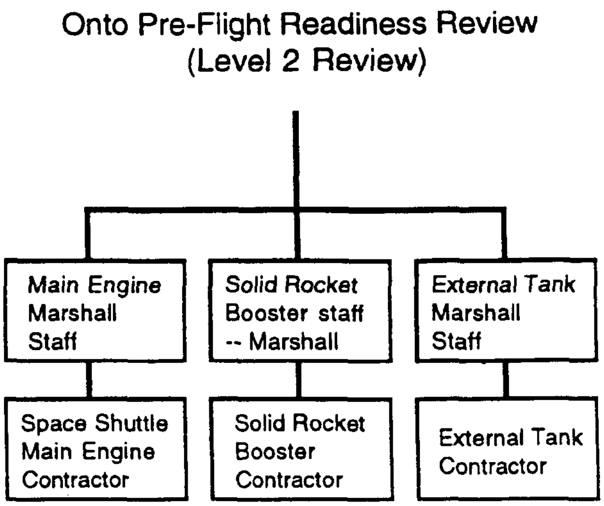
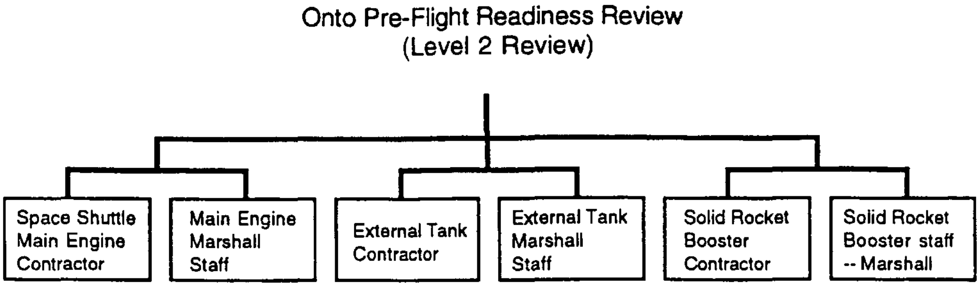

Onto Pre-Flight Readiness Review
(Level 2 Review)
Main Engine Marshall Staff
Solid Rocket Booster staff -- Marshall
External Tank Marshall Staff
Space Shuttle Main Engine Contractor
Solid Rocket Booster Contractor
External Tank Contractor

**FIGURE 8**

**An Unofficial Change in the Structure of the Flight Readiness Review at the Marshall Space Flight Center**


Onto Pre-Flight Readiness Review
(Level 2 Review)
Space Shuttle Main Engine Contractor
Main Engine Marshall Staff
External Tank Contractor
External Tank Marshall Staff
Solid Rocket Booster Contractor
Solid Rocket Booster staff -- Marshall

all parties agree to forward the necessary paperwork. Through his personal supervision of the launch decision process and his domineering style of management, Lucas was able to ensure that this unofficial structure prevailed.

Following the investigation of the *Challenger* accident, it was widely publicized that Marshall solid rocket booster managers had coerced engineers at Morton Thiokol into agreeing to a launch they opposed. Such pressure, however, has worked in both directions. In January 1985, solid rocket booster manager Larry Mulloy sent an urgent memo to Thiokol concerning O-ring erosion. A week later, in the review for shuttle mission 51-D, Thiokol's launch decision was to "accept risk" (Presidential Commission on the Space Shuttle *Challenger* Accident 1986, vol. 1). Once this opinion was expressed, the ability of the Marshall staff to stop the launch was extremely limited. In sum, the launch decision structure at Marshall had been transformed from a serial system, which would require both parties to authorize the flight readiness of the equipment, into a parallel structure needing only one component to approve the launch.

This structural shift had a profound impact on the reliability of the launch decision process at Marshall. Table 2 (Set C) contrasts the organizational reliability of each system, assuming once again, that each component commits type I and type II errors with a probability of .05 and .15, respectively. The new parallel structure reduces the probability that any subsystem will commit a type II error from 27.75% to 2.25%. Curtailing this form of error was completely consistent with Marshall's requirements at the time. The converse is that the probability of a type I failure in each subsystem increases from .25% to 9.75%. The probability that at least one subsystem at the Marshall center would commit a type I error rose from .75% to 26.49% under the new process.

As noted, NASA had changed its emphasis from type I to type II reliability. To consider the impact of this transformation, Table 2 (Set D) compares the two structures with the component probabilities of type I and type II errors reversed. Even when biased toward type II reliability, the serial decision structure is able to keep type I error rates at a low 2.25% in each subsystem. In contrast, the new parallel organization increases this failure rate to 27.75%. The prescribed serial structure clearly limits the possibility that such organizational changes would lead to fatal type I mistakes, a feature that is lost in the unofficial launch process. In light of this analysis, perhaps the most surprising aspect of the *Challenger* accident was that it did not happen earlier.

Since then, the launch decision process at Marshall has been restored to its previous status. In addition, the Office of Safety and Mission Quality has been given a direct voice in the launch decision at the flight readiness review. This office now has the authority to stop any launch that it believes is unsafe, linking it to the system in a serially independent manner. This authority has been exercised on previous launch attempts when the office was concerned with hydrogen leaks on the shuttle, defective door lug-bolts, and other technical problems. On the whole, the launch decision system is now structured to be more protective against type I failures and prevent another mishap like the *Challenger*.

**TABLE 2**

**Probabilities of Failure for Marshall Launch Decision Structures (all results expressed as a percentage)**

| | SET C | | SET D | |
|---|---|---|---|---|
| COMPONENTS & STRUCTURES | TYPE I | TYPE II | TYPE I | TYPE II |
| Component Failure | 5.0 | 15.0 | 15.0 | 5.0 |
| *Serial Structure* | | | | |
| Subsystem Failure | 0.25 | 27.75 | 2.25 | 9.75 |
| Center Failure | 0.75 | 62.29 | 6.60 | 26.49 |
| *Parallel Structure* | | | | |
| Subsystem Failure | 9.75 | 2.25 | 27.75 | 0.25 |
| Center Failure | 26.49 | 6.60 | 62.29 | 0.75 |

*Note*: A Type I error for NASA would be a decision to launch an unsafe mission. A Type II error would be a decision to abort a technically sound mission.
The figure for component reliability, as well as the calculations which follow, are not empirically derived estimates but rather they are assumptions made for the purpose of illustration.

## CONCLUSION

Several conclusions can be drawn at this point. First, organizational structure can have an important impact on administrative reliability. I have demonstrated, both in theory and for the case of NASA, that changes in the number and alignment of administrative components alters the probability that an agency would commit either a type I or type II error. Perhaps most interesting is the fact that NASA changed its institutional configuration to appease both the traditional and the Landau schools of thought in public administration, yet both decisions contributed to the biggest failure in NASA history. With regard to its reliability-and-quality-assurance function, NASA followed the traditional public administration prescription by eliminating redundancies and streamlining its organizational system. At the same time, the space agency also heeded the advice of Landau by creating parallel linkages in the launch decision structure. As I have shown, however, both those decisions were incorrect and contributed to the destruction of the *Challenger*.

While recognizing its limitations, we should not be overly critical of the work in this field by Landau and his colleagues. It is clear that Landau's major objective at the time was to respond those in the discipline who emphasized efficiency over the pursuit of reliability. As such, his work was path-breaking; and his focus on two-state (operating/failed) devices was understandable. As I have shown here, however, the three-state world gives us a theoretically richer and more empirically useful framework for evaluating organizational reliability. Expanding this subject to allow for multiple types of error is an important step toward a more general theory of organizational reliability and agency behavior.

Using this theoretical framework, it is also clear that the agency's choice of structure can provide insights on its priorities. The NASA case is particularly illustrative of this point. In the Apollo era, the agency pursued type I reliability more than type II. To do this, they developed a large serial structures in both the reliability-and-quality-assurance function and in the launch decision process. Over time, there was greater demand for type II reliability, which was met through a series of structural changes in the agency. The intense criticism following the *Challenger* led NASA to radically shift its structure to a system that was even more effective against type I errors. That structural changes mirrored the shifting demands for each form of reliability does not surprise us. As Alfred Chandler, the eminent management historian, once put it, structure follows strategy (Chandler 1962). By looking at the structural modifications within an agency and understanding how these changes influence organizational reliability, we can gain some insight into the agency's true preferences and priorities with regard to different types of reliability.

It is possible to perform such an analysis even if we do not know the exact values of component reliability for both types of error. In the theoretical section, I assumed that the reliability of each component was known. When analyzing NASA, however, I was still able to apply these principles by making some reasonable assumptions about the reliability of the components. More generally, when circumstances dictate greater effort toward one form of reliability, we can use these principles to develop a set of structures that are relatively better at minimizing the error of concern. The theory can also be used to recognize potential weak points in more complex organizations and allow us to reinforce them with regard to the particular error of concern.

Explicitly incorporating multiple forms of error in our analysis will open up new avenues in the public administration research agenda. For example, more work could be done on the study of agency incentives to pursue different forms of reliability. This work helps us to understand *how* agencies may adjust their structural design to meet the demands for different forms of reliability. The question of *why* agencies make the choices they do is one that would be of great interest to political scientists. As Moe (1990) notes, structural choice is, at heart, a political issue. This is particularly true when we recognize that agencies, endowed with limited resources, must make trade-offs between each form of reliability. These choices have political consequences and are influenced by political factors.

Further research could also be done comparing component and systems reliability. Till now, the focus has been on the system as a whole, not the individual components. Concentrating on component issues raises a whole new set of questions. What factors affect component reliability? To what extent is component reliability influenced by the strategic behavior of agents? To what extent can and should agencies pursue reliability objectives through increased component reliability, rather than through structural changes? Answering these questions will also contribute to the development of a more general theory of organizational reliability and agency behavior.

In the end, we must recognize that a theory of organizational reliability is not sufficient to eliminate risks altogether. Space exploration is still a risky business—it always has been and always will be. The men and women who have gone into space understood these risks. Even with the technological advances resulting from Project Apollo and the organizational changes after the *Challenger*, there remains a large element of danger in manned space flight. According to a report by the Office of Technology Assessment in April 1990, there is a 50% chance of losing another orbiter over the next 34 missions. Understanding organizational reliability cannot eliminate these risks altogether, but it can help to ensure that NASA's next technical problem is not compounded by managerial mistakes.

## APPENDIX

### Proof That a k-out-of-m Unit Network Reduces to Serial System Where k = m

Since the success/failure of each of the m units can be modeled as a Bernouilli process, we can use the binomial distribution to describe the system reliability for a k-out-of-m unit network. Letting f represent the probability of failure for each independent unit, the overall reliability of a k-out-of-m unit network may be represented mathematically as

$$R_{k/m} = \sum_{i=k}^{m} \binom{m}{i} (1-f)^i f^{k-i}.$$

When k = m, this expression reduces to

$$R_{k/m} = \binom{m}{m} (1-f)^m f^{m-m},$$

which further reduces to $R = (1 - f_i)^m$. Note that this expression is equivalent to equation 1, which represents the reliability of a serial system in a two-state world.

### Series Systems Optimization

The system reliability of a series network consisting of m identical and independent components is $R_s = (1 - \beta)^m - \alpha^m$. Differentiating this equation with respect to m and setting the result equal to zero will give us the optimal number of components to be linked in series in order to maximize the system's reliability:

$$\frac{\partial R}{\partial m} = [(1-\beta)^m \, ln(1-\beta)] - [\alpha^m \, ln\alpha] = 0$$

$$(1-\beta)^m \, ln(1-\beta) = \alpha^m \, ln\alpha$$

Taking the natural log of each side and grouping like terms,

$$m\left(ln \frac{(1-\beta)}{\alpha}\right) = ln\left(\frac{ln\alpha}{ln(1-\beta)}\right)$$

$$m^* = \frac{ln\left(\dfrac{ln\alpha}{ln(1-\beta)}\right)}{\left(ln \dfrac{(1-\beta)}{a}\right)}$$

### Parallel System Optimization

The system reliability of a parallel network consisting of n identical and independent components is $R_s = (1 - \alpha)^n - \beta^n$. Differentiating this equation with respect to n and setting the result equal to zero will give us the optimal number of components to be linked in parallel in order to maximize the system's reliability:

$$\frac{\partial R}{\partial n} = [(1-\alpha)^n \, ln(1-\alpha)] - [\beta^n \, ln\beta] = 0$$

$$(1-\alpha)^n \, ln(1-\alpha) = \beta^n \, ln\beta.$$

Taking the natural log of each side and grouping like terms,

$$n\left(ln \frac{(1-\alpha)}{\beta}\right) = ln\left(\frac{ln\beta}{ln(1-\alpha)}\right)$$

$$n^* = \frac{ln\left(\dfrac{ln\beta}{ln(1-\alpha)}\right)}{\left(ln \dfrac{(1-\alpha)}{\beta}\right)}.$$

## Notes

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1. A three-state device is a unit that can be described as either operating correctly, committing a type I error, or committing a type II error. One might argue that what we have here are four-state devices, inasmuch as there are two types of correct decisions, as well. While this is true, the focus of reliability theory has historically been on the analysis and impact of failure. As a result, it is the norm to refer to multiple error units as three-state devices.

2. While it is later relaxed, making this assumption is not unreasonable. Measuring personnel productivity is a subject that has been discussed in industrial engineering and other areas of management science. Research from these areas make it possible to generate statistically sound estimates of component performance and reliability.

3. The reliability-and-quality-assurance function at NASA refers to those offices and individuals specifically charged with agency oversight on matters of reliability and safety. This function is not limited to a single office and has undergone a number of changes over time.

4. The Soviets' control over the media allowed them to limit their coverage to the successful flights. In contrast, the United States' first attempt to launch a satellite was a highly publicized failure. The United States learned from this embarrassing experience and tried to minimize press coverage in later launch activities. Such news can rarely be kept a complete secret in our society, however; and the continued contrast between Soviet success and American failure had a political impact at home and abroad.

5. It may well be that considering the policy over a longer time horizon would demonstrate the cost-effectiveness of type I reliability. However, the American political system is notorious for its myopic approach to many public policy issues. After the *Challenger*, many experts were critical of the space agency's shortsightedness and claimed that such behavior was irrational. If the political environment of the agency is factored in, the policy outcomes may seem undesirable; but they are not irrational from the agency's perspective (Heimann 1990, 1991).

6. While these two offices had overlapping responsibility, the Reliability and Quality Assurance Office was primarily concerned with hardware and technological issues, while the

Safety Office focused on human factors and included responsibility for worker safety procedures.

7. Special Announcement, February 6, 1973, "Establishment of Office of Safety and Reliability and Quality Assurance," NASA History Office Library.

8. Figures on reliability-and-quality-assurance manpower were calculated from numbers provided to Congress by the chief engineer and by the Office of Safety and Mission Quality and the Office of Human Resources at NASA.

9. Mark Tapscott, "Cuts Hurt NASA's Safety," *Washington Times*, 5 March 1986.

10. Directors at each center have traditionally been given flexibility to set R&QA staffing and funding for projects under their control. Before the *Challenger*, R&QA officials at headquarters could make recommendations about center staffing, but had no direct control over this process so as to reinforce the independent nature of the field center unit.

11. This assumption would be consistent with the belief that NASA is generally risk-averse with regard to type I error. As noted earlier, however, increasing type I reliability often raises the likelihood that a type II error could occur.

12. News Release, 5 March 1974, no. 74-31, NASA History Office Library.

13. Memorandum, assistant associate administrator for management operations to associate administrator for management operations, September 7, 1978, "Institutional Assessment Presentation to OMB," NASA History Office Library.

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# RESEARCH NOTES

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# THE DYNAMICS OF TRADE UNIONISM AND NATIONAL ECONOMIC PERFORMANCE

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*I test two theories of the political processes of trade unions. The first argues that wage moderation depends on a centralized labor movement. The second contends that, institutional conditions permitting, unions' coordination of bargaining strategies is sufficient. Coordination is most likely to be achieved when there are small number of unions that do not compete for members, that is, when union monopoly is high. Important empirical anomalies may be resolved by analyzing the effects of union centralization and monopoly separately, rather than combining them into a composite index of corporatism. Reanalyzing comparative data from Organization of Economic Cooperation and Development countries between 1963 and 1985 largely corroborates the hypothesis that monopoly is more important than either centralization or composite indices of corporatism for national economic performance. The conceptual rationale underlying indices of corporatism should be reexamined.*

Extensive literature in political science and political economy demonstrates a statistical association between aspects of union structure and measures of national economic performance (Alvarez, Garrett, and Lange 1991; Armingeon 1987; Bean, Layard, and Nickell 1986; Blaas 1982; Bruno and Sachs 1985; Calmfors and Driffill 1988; Cameron 1984; Crouch 1985; Garrett and Lange 1986; Hicks 1988; Lange and Garrett 1985; Lutz 1981; McCallum 1986; Marks 1986; Newell and Symons 1987; Soskice 1990; Wallerstein 1990). The intuition motivating these studies is that while many factors must explain the substantial variations in macroeconomic performance characterizing the advanced industrial countries in recent decades, some union movements have been able to achieve greater wage restraint than others, restraint that in turn has facilitated greater price stability and lower unemployment rates. Summarizing much of the literature, Bruno and Sachs hold that the "real wage moderation" characteristic of corporatist union movements "is the key to achieving low inflation and low unemployment after a supply shock" such as the shocks associated with the oil price increases in the 1970s (1985, 217).

Despite impressive statistical success, this line of inquiry has typically failed to elucidate either the underlying, micro processes and dynamics within trade union movements that contribute to better macroeconomic outcomes or, conversely, those that impair them.[1] Instead, statistical analyses have concentrated on the macro structural causes of variations in national economic performance. This has generated controversy about patterns of associations among variables, without according great attention to specifying the mechanisms underlying such patterns. The behavior of trade unions has been undertheorized.

I shall test two theories of the political processes of trade unions: (1) that wage moderation results only with the centralization of the labor movement and (2) that wage moderation may occur without centralization provided institutional conditions facilitate the coordination of bargaining strategies among unions. Coordination is most likely to be achieved when there are small number of unions that do not compete for members, that is, when *union monopoly* is high. Important empirical anomalies may be resolved by analyzing the effects of union centralization separately from those of monopoly, rather than combining them in a composite index of corporatism. Reanalyzing comparative data for Organization of Economic Coordination and Development (OECD) countries between 1963 and 1985 largely corroborates the hypothesis that union monopoly is a more important explanatory variable for national economic performance than union centralization and that it is more important than indices of corporatism. These findings suggest that the conceptual rationale underlying such indices should be reexamined.

I shall lay out two competing theories of the political processes of trade unions, then show that distinguishing the impact of centralization from monopoly improves our understanding of the contemporary German and Swedish cases. I then discuss implications for the concept of corporatism and test the explanatory importance of union centralization and monopoly systematically against each other and against composite indices of corporatism in a cross-national statistical analysis. Finally, I consider implications of the findings.

## THEORIES OF THE POLITICAL PROCESSES OF TRADE UNIONS

Underlying contemporary research on the political economy of advanced industrial democracies are two competing models of the political processes characterizing trade unions: one associated with a focus on union bureaucracy, the other, with the public goods approach to union behavior. These entail fundamentally different theories of how labor's structural attributes affect its organizational behavior and, ultimately, the national economy.

A distinguished tradition of research rooted mainly in the discipline of sociology, depicts unions as undemocratic, bureaucratically insular oligarchies, whose decision making reflects the interests of leaders rather than the led (see Lipset, Trow, and Coleman 1956; Michels 1962; see the review in Golden 1992). According to this line of argument, corporatist success in improving a country's economic performance is driven by national union officials, who, in response to a combination of government pressure and cajoling, underwrite wage agreements that fail to reflect the preferences of the average union member. This view depicts rank-and-file workers as viscerally inclined to resist wage moderation but unable to do so, at least for the moment, because of a national union leadership that has successfully insulated itself from the claims of its members. Illustrating this line of work, Pizzorno (1978) and Panitch (1981) argue that national corporatist structures (which institutionalize what Pizzorno labels the "political exchanges" that link elites representing labor and capital to the government) face chronic instability in the form of rank-and-file wage revolt and grass-roots rebellions.

This account thus seizes on the formal centralization of authority within organized labor as the conceptually central structural attribute determining union behavior and directing its impact on the national economy. It suggests that the size and scope of an economywide labor movement allow for greater internal centralization of authority. If it were not for that, workers would pursue their true interest, open conflict with capital.

An alternative formulation of labor's political processes is offered by studies that stress the collective action problems affecting union movements (Lange 1984; Wallerstein 1990). It may or may not be rational for workers to engage in wage militancy; but even when it is in the interest of workers to restrain militancy, they may find they are unable to do so. Cooperation among workers and among unions may be difficult to achieve because of free-riding problems. Even if workers collectively prefer to hold down wage militancy, it is in no individual worker's interest to do so, since individual restraint in a context of generalized wage militancy makes the individual relatively that much worse off. It is thus necessary to solve labor's collective action problem in order to ease rivalries and enforce cooperation among workers.

In contrast with the first view, the latter argument suggests that coordination among unions may effectively achieve wage moderation, even in the absence of centralization. Indeed, the whole idea that interactions among unions should be thought of as an iterated game undermines the rationale for focusing on centralization. Centralization speaks to the extent to which confederal offices are able to vet wage demands, sanction strike action, and generally disperse union resources. But within union movements in democratic societies, the authority that comes with such instruments of ostensibly coercive control is ultimately voluntary, resting as it does on the ongoing decisions by subordinate union actors to acquiesce to it. Democratic trade unionism almost everywhere entails the autonomy of union bodies to coalesce or secede as they choose. Ultimately, therefore, the authority of central confederations consists in the ability (perhaps) to withhold strike funds and the threat (sometimes) to expel a disobedient union. Certainly, however, where large unions are concerned, a confederation has an interest more or less equal to the union's in seeing the latter's affiliation continue, since expulsion would jeopardize the confederation's own representative position. The authority of central confederations over their affiliates, especially their largest affiliates, is thus inevitably likely to rest more on bargaining and agreement among the parties than on any coercive capacity the confederation may appear to exercise. Institutional arrangements promising centralization among unions are ultimately rarely enforceable.

Instead, the logic of a game-theoretic account suggests that the abilities of multiple unions to coordinate their bargaining behavior is likely to rest on two factors, both captured by the concept of union monopoly, or what is called *concentration* by industrial relations specialists. These are the number of national-level actors involved in the wage-setting process on the union side and the degree of competition likely to characterize relations among them. The first indicates the likelihood that labor overcomes internal coordination problems; the second, the likelihood that it resolves internal distributional conflicts.

### Coordination Problems

The collective action literature has demonstrated consistently that coordination problems, such as prisoner's dilemmas, are more easily overcome when fewer actors are involved (see Olson 1965). A useful analogy for present purposes is Bowman's (1982) analysis of how numbers influence the ability of groups of firms to engage in cartellike pricing behavior. (The reader is asked to make the appropriate translation to the phenomenon of wage setting among unions.)

> As the number of firms in the industry increases, the likelihood that they will successfully cooperate without outside intervention decreases. This is true for several reasons. First, as the number of actors increases, the costs of coming to an agreement on a collusive strategy will increase. . . Second, an increase in numbers increases the difficulty of transmitting and interpreting the information upon which the cooperative effort depends. . . Third, as numbers increase, so does the likelihood that the industry will contain a "maverick" firm determined to torpedo any effort to maintain price stability for ideological or psychological reasons. Fourth, individual firm size held constant, as the number of sellers increase, the impact of each firm's choice of strategies on the total outcome is diminished, tending to render the detection of non-cooperative behavior more difficult. . . . This will also mean that firms will cease to

> recognize their interdependency and will select their strategy according to non-strategic criteria, that is, they will reduce prices simply because it is the rational thing to do. (p. 583)

Union movements confront similar problems in wage setting. Where small numbers are involved, unions can monitor and assess their own and each other's behavior. Fewer numbers eases their coordination problems in moderating wage demands, thereby reducing inflationary pressures and maintaining employment.

### Distributional Conflicts

Not all union problems are simply coordination problems that can be eased by reducing the number of actors involved. Inter- and intraunion relations are also characterized by genuine conflicts of interest. The underlying dynamic is competition for members, which provides a strong incentive for unions to try to maximize their wage gains in order to retain members or to attract them away from competitors. Demarcating nonoverlapping union territories eases such distributional conflicts. At the same time, fewer and (ceteris paribus) larger unions set wage rates for larger groups of workers. This depresses the visibility and organizational importance for workers of wage relativities, which have, in turn, typically been implicated in the generation of patterns of "leapfrogging" among unions. When unions do not compete for members, they have fewer incentives to engage in wage militancy.

Thus, there are good reasons to believe that where unions are few in number and do not compete for members (i.e., where union monopoly is high), they will be better able to coordinate their wage behavior in ways ultimately beneficial to the national economy.

## TWO EMPIRICAL PUZZLES

The breakdown of centralized collective bargaining in Sweden in the 1980s and the relative success of German wage restraint in the 1970s illustrate the importance of distinguishing the putative impact of union centralization from monopoly. Without such a distinction, outcomes in these cases remain anomalous.

Swedish experts agree that the industrial relations system consolidated in the 1950s broke down irreversibly over the course of the 1980s. The first breach in what had until then been considered an extremely centralized blue-collar confederation, the Landsorganisationen i Sverige (LO), came in 1983–84, when business successfully concluded a separate agreement with the LO's metalworkers' union (Lash 1985). Although peak-level bargaining resumed temporarily in the years following, it had been permanently eclipsed by 1990, as signaled by the decision of the leading employers' association, Svenska Arbetsgivareföreningen, to dismantle its own bargaining unit and to leave individual employers to bargain as they chose (Pontusson 1992).

On the face of it, this sounds like a straightforward collapse of the binding authority exercised by Sweden's largest confederation, the LO. But the impetus for these changes in relations among various unions affiliated with the LO came from outside the LO altogether; it came from competing confederations.[2] The 1960s and 1970s had witnessed considerable growth of Sweden's two major white-collar unions, Sveriges Akademikevs Central Organisation (SACO) and the Tjänstemännens Centralorganisationen (TCO). Whereas the TCO continued into the 1980s to follow the LO's lead in formulating wage demands, even as the former's membership increased to the equivalent of 75% of the latter's in its private-sector unions, the SACO had already initiated separate bargaining in the mid-1960s. According to various accounts it was heightened competition between the unions affiliated with the LO and its white-collar competitors (especially between public- and private-sector unions) that debilitated the LO's authority within the Swedish labor movement (Ahlén 1989; Lash 1985; Pontusson 1992; Swenson 1992). The collapse of formal centralization, in short, was engendered by the preceding loss of the LO's monopoly position in representing employee interests. As a result, "[the] LO has lost whatever capacity it may ever have had to implement a standard wage rate policy throughout the labor market just by coordinating the wage bargaining of its own affiliates" and today is torn by "inter-union wage rivalry" (Martin 1991: 22). Moreover, in important ways, the ostensible centralization of union authority within the LO had always been fundamentally voluntary (see the evidence presented on pp. 31–32). For instance, constituent unions had to renew on an annual basis their agreement allowing the LO to negotiate wages for them. As a consequence, as Martin notes, the "LO can only play a role in wage bargaining that its affiliates agree that it should" (p. 25). Although the Swedish union movement is generally ranked as among the OECD's most centralized, centralization there was hardly self-fulfilling or self-binding (see Elster 1984, sec. 2).

The Swedish story illustrates one possible outcome of a discrepancy between centralization and monopoly. A growing divarication between the degree of representative monopoly achieved by central organizations and the extent of their authority resulted in the eventual collapse of central authority. By all accounts highly centralized until the late 1980s (it was among the most centralized of European union confederations), fissures in the LO's monopoly position had emerged two decades earlier (see Golden, Lange, and Wallerstein 1992, tbl. 6 and fig. 7). These eventually proved responsible for the breakdown of formally centralized bargaining altogether.

Structural anomalies in the institutional landscape of organized labor also characterize the German case. The demise of the institutions for concerted action at the end of the 1960s would seem to have promised a turn for the worse for the German economy. Certainly, judging by the degree of trade union centralization, the country appeared headed for difficulties, since the 17 industrial unions affiliated with the major union confederation, the Deutscher Gewerkschaftsbund (DGB), exercised complete bargaining autonomy and the DGB itself officially lacked just the kind of authority characteristic, for instance, of the LO during the same period (see Golden, Lange, and Wallerstein 1992, fig. 5). Despite these inauspicious structural circumstances, "macroeconomic policy in West Germany was less exposed to the pressures of wage-push inflation in the mid-1970s than in any other Western European country" (Scharpf 1984, 281). In part as a result, during the 1970s and 1980s, Germany experienced considerably better than average inflation rates and even somewhat better than average unemployment rates compared with other advanced capitalist countries (Hennings 1982, 498).[3] In other words, the German economy outperformed the expectations associated with the low degree of union centralization found there. This is especially true for the rate of inflation, which is precisely where the greatest impact of the wage aggressiveness that should have resulted from a relatively decentralized labor movement should have been felt.

Various explanations for Germany's low inflation have been advanced, including the critical role of the central bank in restraining the growth of the money supply (Scharpf 1984, 282–85). The surprising degree of union wage moderation has also been observed. Given its relatively low degree of centralization, German labor should have pursued an aggressive strategy of wage militancy during the 1970s (p. 285). That it did not was thanks to the very high degree of coordination achieved by the country's small number of monopolistically organized industrial unions (see Streeck 1981, 152–53; Golden, Lange, and Wallerstein 1992, tbl. 2). German collective bargaining in the 1970s and 1980s reflected a process of voluntary cooperation among unions. The national federations allowed the largest of them, the metalworkers, to act as a patternsetter, thereby achieving wage coordination and restraint despite an absence of central DGB authority (Thelen 1991, 40–41). The metalworkers, Europe's largest single union federation, had strong incentives to restrain wage growth in order to retain jobs in major industries, such as steel and automobiles, thereby protecting their own membership levels (Soskice 1990, 44–45).

Thus, an extremely concentrated union movement (e.g., the German) may achieve considerable wage restraint even without much internal centralization. By reducing wage rivalry among unions and allowing one national federation to act as a patternsetter, a high degree of union monopoly may spontaneously allow the coordination of wage demands. Even without much coercive central authority, labor may overcome its collective action problem.

## THEORETICAL IMPLICATIONS FOR THE STUDY OF CORPORATISM

The corporatist literature turns on the idea that to the extent that organized labor acts as a unitary and economywide actor, it makes decisions with reference to the macroeconomic consequences of its own actions (Olson 1982). Characteristic in this regard is Scharpf's (1984) depiction of the importance of various attributes of union structure for corporatist successes in the key realm of wage restraint:

> The institutional capacity of a country to have a voluntary incomes policy seems to depend primarily upon the strength and the organizational structure of its labor unions and upon the effective locus of collective bargaining. If union organization is concentrated, with a limited number of "encompassing" industrial unions, and if collective bargaining is centralized at the national level, the unions will tend to consider the macroeconomic consequences of wage settlements out of rational self-interest even in the absence of government guide-lines. . . . On the other hand, if union organization is fragmented and collective bargaining decentralized, individual wage settlements are unlikely to be determined by considerations of macro-economic management. (p. 275)

Thus, the concept of corporatism deliberately merges the extent of centralization within the labor movement and the concentration of organized labor (what I earlier identified as union monopoly) and often the extent of unionization, as well.

The Swedish and German cases illustrate, however, that these various attributes of union structure do not necessarily go together. Union centralization and union concentration, for instance, may move in opposite directions.[4] Empirically, there is reason to analyze these components of corporatism separately. There are also theoretical reasons to question the underlying logic of corporatism. I have already suggested that the logic of a game-theoretic account seems well captured by the concept of union monopoly, whereas the role of centralization is conceptually more problematic. As an explanatory causal variable, centralization improperly treats an endogenous variable as exogenous. Union centralization is itself an outcome of cooperation among unions, not an independent cause of their ability to cooperate. There is no theoretical justification for automatically treating an institution as a credible commitment to a particular course of action; institutions, like policies, can be remade if they fail to conform to the interests of their participants. The assumption that centralization is responsible for the kind of cooperation necessary for wage restraint thus requires more justification than it has received.

Second, the Swedish and German cases show that when monopoly and centralization fail to coincide, it is monopoly that does the causal work. It was a breakdown of monopoly that caused a collapse of centralization in Sweden, and it was the high levels of monopoly that permitted successful wage coordination in Germany even in absence of centralization.

This suggests that the role of centralization in wage restraint should be reevaluated.

Finally, there are methodological reasons to unpack corporatist indices and, if possible, to economize with monopoly. Union monopoly is easier to measure cross-nationally than centralization. Standard quantified data exist for monopoly, unlike centralization (see the Appendix for details). Measures of centralization are easily susceptible to problems of intercoder reliability. If they can be discarded on theoretical grounds, there are good operational reasons to do so.

## OPERATIONAL DEFINITIONS OF THE INDEPENDENT VARIABLES

Most statistical studies that use measures of corporatism work with composite indices. These typically comprise a measure of union centralization (used by Armingeon 1987, Bean, Layard, and Nickell 1986, Bruno and Sachs 1985, Calmfors and Driffill 1988, Cameron 1984, Crouch 1985, Czada 1987, Garrett and Lange 1986, Headey 1970, Hicks 1988, Lange and Garrett 1985, Marks 1986, and Wallerstein, 1990); a measure of union concentration, or union monopoly (used by Calmfors and Driffill 1988, Cameron 1984, Crouch 1985, and Schmitter 1981); and sometimes an added measure of union density, or the proportion of employees who join unions from those eligible (used by Armingeon 1987, Cameron 1984, Czada 1987, Freeman 1988, Garrett and Lange 1987, Lange and Garrett 1985, Olson 1982).

*Union centralization* refers to the degree of internal authority exercised by union confederations over lower organizational bodies. By *confederation* I mean labor's central peak association—the Trades Union Congress (TUC) in Britain, the AFL–CIO in the United States, the LO in Sweden. Comprised of individual national unions (in some countries called national federations), confederations are multisectoral, economywide bodies engaged to varying degrees in collective bargaining, consultation and negotiations with government, international liaison activities, and the like.[5] I shall refer to them interchangeably as confederations and central union organizations.

Substantial variation exists in the extent of confederal monitoring and control over constituent units. In an extremely decentralized union movement, for instance, national industrial unions as well as union locals may be empowered to call strikes and sign agreements at their own discretion. In a highly centralized union movement, by contrast, the central confederation alone officially calls strikes and may also have the authority to vet wage agreements, which extend to entire industries if not, in practice, to the whole economy. In such systems, local bargaining may not even occur, or if it does, local bargaining agents may be acting under the direct supervision of (or perhaps in tandem with) higher-level officials.

Union monopoly, a measure of the degree of fragmentation of organized labor, also exhibits marked variation in the advanced capitalist democracies. In many—indeed, most—of these countries, multiple confederations exist. Distinguished by ideological (socialist vs. nonsocialist) or religious (Catholic vs. Protestant) orientations or by broad occupational or status differences (manual vs. white-collar), the presence of multiple confederations poses distinct organizational problems unrelated to those connected to the degree of authority maintained by single confederations. In a highly concentrated union movement, a single confederation exists, enjoying a monopoly of representation for the functionally defined groups it represents. In an extremely fragmented union movement, by contrast, there may be three or more confederations competing across functional boundaries for members. At the same time, the number of national unions affiliated with particular confederations also varies substantially. In the late 1970s, for instance, the British TUC had over a hundred affiliates, whereas the German DGB had only sixteen (Windmuller 1981, 49, tbl. 2).

Density, finally, is a measure of the unionization rate, or the proportion of persons who join unions out of the total pool of employed persons. Unionization rates in the OECD countries range widely, from less than 15% in the United States to more than 80% in Sweden. Low levels of density mean that much wage setting is occurring outside of collective bargaining.[6]

## MEASUREMENT OF THE INDEPENDENT VARIABLES

Measurement problems permeate the corporatist literature. One indication of the absence of standard measures is the fact that for reasons often not fully spelled out, individual studies have tended either to make minor adjustments to measures drawn from earlier studies or to devise new indices altogether. Virtually every study, in consequence, employs slightly different measures. Lijphart and Crepaz (1991) now find 12 extant indices of corporatism in the literature. Given the small number of country cases available for analysis (a dozen or two), apparently minor measurement alterations are likely to affect statistical results.

At the same time, the measures used in all studies reflect common shortcomings. The most serious is that summary measures for attributes of trade unionism telescope the period since World War II, even though union movements in most advanced capitalist countries have experienced significant changes in organizational form over the last half century. Both of these problems (the idiosyncratic and apparently ad hoc alteration of data by individual scholars and the temporal crudeness of the summary measures employed) are testimony to the absence of a standard data set spanning the entire postwar period.

While my colleagues and I are currently constructing such a data set (Golden, Lange, and Wallerstein, n.d.), I have in the meantime chosen to work with measures provided by existing studies. My aim is to focus attention on theoretical problems and to develop an argument that holds regardless of the idiosyncracies of the measures used. In particular, I seek to dispel suspicion that findings may be artifacts of any particular data set or of specific measures. For this reason, I have chosen neither to devise new measures nor to alter data drawn from what may be considered standard sources.

The two sets of measures most often adopted by others are due to Cameron (1984) and Schmitter (1981), each of whom include measures of union centralization and monopoly, as well as individually constructed composite indices of corporatism (for details, see the Appendix). Although neither work avoids the two problems identified, both have gained widespread acceptance. Arguably, they exhibit only legitimate differences in expert judgment. It thus seemed desirable to gain the benefits provided by using two equally reputable indices for the same variables. At the same time, these are the only two studies I was able to identify that independently constructed measures for both union centralization and concentration (as well as indices of corporatism).[7]

How well do the various independent variables provided by Cameron and Schmitter stack up against each other? Their two measures of centralization (r = .89) and their two measures of monopoly (r = .85) are strongly correlated with each other, showing that Cameron and Schmitter appear generally to agree about ordering national union movements for degrees of union concentration and centralization.[8] Additionally, highly centralized union movements tend to be highly concentrated according to Cameron (r = .77), rather less so according to Schmitter (r = .50). Finally, the measure of union density provided by Cameron is strongly correlated with his measure of centralization (r = .73), but less with his measure of concentration (r = 56).[9]

Not surprisingly, both analysts find that corporatism is strongly associated with union centralization and with union monopoly. Correlation coefficients for corporatism and centralization are .89 for Cameron's measures and .82 for Schmitter's; for monopoly, they are .81 and .88, respectively. Finally, the indices of corporatism that they construct are highly associated with each other (r = .87). On the face of it, not much differentiates the two sets of measures.

## DATA AND INDICATORS FOR ECONOMIC PERFORMANCE

I use four measures of the dependent variable, economic performance. These are the rate of unemployment plus the rate of inflation (the "misery index"), separate measures for unemployment and for inflation, and an alternative performance index comprising the rate of unemployment plus current account deficit in percentage of gross domestic product. This last measure is designed especially to take into account open economies, where the appropriate measure of unemployment is that consistent with current balance equilibrium (for a discussion, see Soskice 1990, 57).

I selected these measures of national economic performance (the dependent variable) for two reasons. First, they have been often used in the relevant literature, being obviously related to what we mean by economic performance. Thus, they are likely to be relatively uncontroversial. Second, the impact of trade unionism is likely to be comparatively larger and more visible on these measures than others (e.g., growth rates). This is especially the case for inflation, since union action has a direct bearing on wages; but it is also likely to be true for unemployment rates, given the relationship between wage levels and the number of jobs. Other measures of national economic performance on which data are easily available are more remote from trade unionism and reflect greater influences of various other factors. Since the point of the present exercise is to unravel how trade unionism affects economic performance and not to explain variations in economic performance per se, it seemed advisable to use measures of the dependent variable that were relatively closely related to union action.[10]

For all four variables, I use OECD data reported by Calmfors and Driffill (1988, 20, tbl. 2). Calmfors and Driffill offer two different measures of each variable for seventeen countries:[11] averages between 1974 and 1985 and averages for these years compared with averages for 1963–73. While precise periodization may differ across studies, operationalizing economic performance by period is a common strategy and thus also likely to be uncontroversial. Levels and changes (as Calmfors and Driffill refer to them) are in all cases highly correlated.[12] I shall report both.

## TESTING HYPOTHESES OF LABOR'S POLITICAL PROCESSES

I now test the two theories of the political processes of trade unions. Does wage moderation result (1) only with the centralization of the labor movement or (2) when the institutional conditions associated with high levels of union monopoly permit unions to coordinate their bargaining strategies? I also examine whether union monopoly may not perform better than the composite measures of corporatism of which it usually forms a part.

Since the literature offers competing specifications of the relationship between trade unionism and economic performance, tests were conducted with different models. A first wave of work proposed a linear model, arguing that more corporatist countries experienced better economic performance than their less corporatist counterparts (Bruno and Sachs 1985; Cameron 1984; Crouch 1985). The reasons go back to the kind of analysis I have discussed: corporatist union leaders are more willing and more able to behave responsibly in collective bargaining. More recently, however, a second wave of studies contends that the relationship between corporatism and economic performance is parabolic and that both the most and the least corporatist countries are characterized by economic performance superior to middling nations. The theoretical rationale for this latter argument is owed to Calmfors and Driffill (1988), who have analyzed different institutional regimes of wage bargaining, arguing that highly centralized union movements, as well as enterprise unionism each have the effect of restraining wage increases. Intermediate union movements (those characterized largely by industrial unionism, in Calmfors and Driffill's account) are those most likely to see large wage increases, which in turn spiral into more generalized inflationary problems.

A complementary line of argument has been advanced by analysts concerned with the relationship between trade unionism and left-wing political power. A series of articles claims that countries with both centralized unions and left-wing governments, as well as those with decentralized unions and conservative governments, outperform those with mixed characteristics (Alvarez, Garrett, and Lange 1991; Garrett and Lange 1986; Lange and Garrett 1985). This work contends that the behavior of trade unions is conditional upon the political environment in which unions find themselves. While the spirit of their analyses differ, the findings corroborate the results reported in Calmfors and Driffill's study showing that the relationship between trade unionism and macroeconomic performance is parabolic.

Finally, a recent study by Soskice (1990) attempts to rehabilitate the original linear hypothesis by looking at the degree to which wage bargaining is nationally coordinated, rather than whether the union movement is formally centralized. Because he recodes some important countries, Soskice's empirical test produces statistically impressive results challenging nonlinear specifications.

Since the goal of the present analysis is to assess which aspects of trade unionism bear on the macroeconomy, not to evaluate competing hypotheses about the nature of the relationship between organized labor and the economy, I test both linear and nonlinear models. My results thus fall into three parts: (1) results of linear specifications of the model; (2) results of analyses testing nonlinear specifications, both hump-shaped and the politically conditional model associated with Lange and Garrett; (3) results of an analysis of the importance of economywide wage coordination, with a more detailed discussion of Soskice's study.

## RESULTS OF A LINEAR MODEL

The first model assumes that linear relationships best capture the link from union structure to national economic performance. Bivariate regression analysis over 14 observations corroborates the findings reported in numerous other studies: union centralization, monopoly, and overall measures of corporatism are positively associated with various indicators of macroeconomic performance.[13] Results differ according to which data set is used to assess the relative importance of these three independent variables, however. Cameron's data produce results nearly uniformly favoring monopoly over centralization, as well as over his composite index of corporatism.[14] In seven of eight groups of equations, the standardized coefficients for monopoly are higher than those for either centralization or corporatism; and in the eighth, the difference is in the second decimal place. Five of the eight equations for monopoly achieve levels of statistical significance at the .10 level or better. Corporatism and centralization, by contrast, attain equal levels of significance only once each.

Initial results from Schmitter's data, however, tend to favor centralization over monopoly. Of 24 equations, eight achieve significance at the .10 level or better. Four of these use corporatism as the independent variable, and four use centralization. Standardized coefficients are highest for union centralization in three equations and for corporatism in five.

Least squares regression is, however, known not to be robust (e.g., Rousseeuw and Leroy 1987).[15] A very small number of discrepant cases (even one) can mask patterns in the bulk of the data. The detection and deletion of outliers can be justified on such a basis; that is, outliers must first be removed if regression coefficients are to provide a summary of a set of data. Given the contrasting results obtained using Cameron's and Schmitter's measures, removing outliers is one way to reanalyze the data.

I used least median of squares (LMS) regression to detect for outliers. LMS is a very robust regression procedure that is resistant to outliers in the $x$-, as well as the $y$-direction. This is its chief advantage over other robust estimators. Rousseeuw and Leroy (1987) recommend using the scaled residuals from LMS regression for outlier detection. I repeated the bivariate regression analysis just reported using LMS instead of least squares regression. Inspection of the scaled LMS residuals revealed Switzerland to be a consistent outlier for many different specifications. While other outliers were identified at various points in the analysis, Switzerland was the only case that deviated for most models.

I therefore performed bivariate regressions with that case omitted. Removing Switzerland does not materially affect the results obtained using Cameron's data set. The standardized coefficients for monopoly continue to be stronger than those for centralization in every instance and are also stronger than the coefficients for his index of corporatism in all but two instances. Six of eight bivariate regressions for monopoly produce results statistically significant at the .10 level or better, compared with only one statistically significant equation using centralization and none using corporatism.

Omitting the Swiss outlier noticeably affects the results for the analysis using Schmitter's data. In five of the eight sets of equations, coefficients for monopoly are now larger than for union centralization. Moreover, half of the bivariate regressions using monopoly now obtain acceptable levels of statistical significance. Not surprisingly, given that Schmitter's index of corporatism is an additive index comprising ordinal rankings of countries on union centralization and monopoly, the coefficients for corporatism are higher in bivariate analyses than for either of its components, even with the Swiss outlier omitted.

To evaluate the validity of these results, I next undertook a bootstrap analysis. The bootstrap is a general method for making statistical inferences in the absence of strong assumptions about the parameters of the stochastic component of a statistical model. The bootstrap involves resampling from a data set with replacement and calculating a statistic from the resampled data. This procedure is repeated hundreds of times to obtain a bootstrap distribution. Assuming that the data form a representative sample, this distribution can be interpreted as the sampling distribution of the statistic. Efron (1982) has shown that when classical conditions are fulfilled, the bootstrap yields classical inferences.

If a data set is large, limit theorems will often justify the properties of inferential statistics. With small data sets, however, departures from assumed distributions in the data may lead to incorrect inferences (see Judge et al. 1985, 153 for a discussion of the consequences of nonnormal residuals in least squares regression with small samples). Furthermore, in a few situations, analytical results do not exist for the sampling distribution of some statistics. The difference between two regression coefficients with unknown heterogeneous variances is such a situation. In these circumstances, test statistics can be obtained empirically with the bootstrap.

In the bivariate regressions just reported, my chief interest lies with the difference between the centralization and monopoly coefficients, rather than their magnitudes. The hypothesis is that the monopoly coefficient is larger than the centralization coefficient. To test this, I resampled with replacement from a small data set consisting of the two independent variables and a dependent variable. For each bootstrap sample, I calculated two bivariate coefficients. The statistic of interest is the difference between the two coefficients, which indicates how much more important one independent variable is than the other. I drew two hundred bootstrap samples. In Table 1, I report the median, the significance level, and the 80% confidence region of the bootstrap distribution of the differences between monopoly and centralization. Where the median difference is positive and the confidence interval does not overlap zero (or at least not by much), we can be confident that the monopoly coefficient is larger than the centralization coefficient, given stochastic variability.

TABLE 1

**Bootstrap Analysis of Difference, Union Monopoly and Centralization**

| DEPENDENT VARIABLE | CAMERON'S MODEL | SCHMITTER'S MODEL |
|---|---|---|
| Avg. level 1974–85 | | |
| Unemployment | .11 | .20 |
| | (.23) | (.23) |
| Inflation | .29 | −.07 |
| | (.12) | (.29) |
| Misery | .26 | .05 |
| | (.09)* | (.42) |
| Altern. perf. index | .12 | .25 |
| | (.32) | (.25) |
| Avg. 1974–85 − avg. 1963–73 | | |
| Unemployment | .06 | .29 |
| | (.42) | (.19) |
| Inflation | .29 | −.05 |
| | (.07)* | (.37) |
| Misery | .27 | .15 |
| | (.07)* | (.21) |
| Altern. perf. index | .07 | −.09 |
| | (.38) | (.37) |

*Sources*: For Cameron's and Schmitter's measures of monopoly and centralization, see the Appendix; unemployment, misery, and alternative performance index from OECD are reported in Calmfors and Driffill 1988, 20, tbl. 2; inflation is calculated as misery minus unemployment. The alternative performance index is the unemployment rate plus current account deficit in percentage of gross domestic product.
*Note*: Bootstrap median; significance level is in parentheses. N = 13.
*$p \leq .10$, one-tailed test.

Results largely corroborate the hypothesis favoring union monopoly. The median difference is positive for all eight dependent variables with Cameron's data set and for five of the eight with Schmitter's. This analysis tends to confirm that the greater importance of union monopoly over centralization found in the bivariate regressions across 13 cases reported earlier is valid.

Table 2 presents results of multiple regression analysis using Cameron's and Schmitter's different measures of union attributes, both over all 14 cases and with the Swiss outlier omitted. As in the bivariate analysis I have reported, results establish the greater importance of union monopoly compared with union centralization for Cameron's data. In all the equations based on this data set, whether across 13 or 14 observations, monopoly emerges as more important than union centralization. Moreover, the significance levels for monopoly are quite high, especially in view of the comparatively high level of collinearity between Cameron's measures of centralization and of monopoly, providing additional evidence for the importance of this variable. Centralization is not significantly associated with *any* measures of the dependent variable and even exhibits the wrong sign in some cases.

However, collinearity typically influences results

**TABLE 2**

**Linear Regression Models Using Cameron's and Schmitter's Measures of Union Structure**

| | CAMERON'S MEASURES | | | | | | SCHMITTER'S MEASURES | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ALL COUNTRIES | | | SWITZ. OMITTED | | | ALL COUNTRIES | | | SWITZ. OMITTED | | |
| DEPENDENT VARIABLE | CEN-TRALIZ. | MONOP. | $r^2$ | CEN-TRALIZ. | MONOP. | $r^2$ | CEN-TRALIZ. | MONOP. | $r^2$ | CEN-TRALIZ. | MONOP. | $r^2$ |
| Avg. level 1974–85 | | | | | | | | | | | | |
| Unemployment | .09 | .47 | .30 | .10 | .57 | .42 | .34 | .10 | .16 | .12 | .56* | .39 |
| | (.97) | (5.45) | | (.87) | (5.54) | | (.24) | (.07) | | (.08) | (.37) | |
| | [4.02] | [4.56] | | [3.26] | [3.70] | | [.22] | [.22] | | [.20] | [.19] | |
| Inflation | −.47 | .87** | .34 | −.53 | .96** | .42 | .49 | −.05 | .22 | .42 | .17 | .29 |
| | (−5.36) | (11.09) | | (−5.44) | (11.16) | | (.38) | (−.04) | | (.33) | (.14) | |
| | [4.33) | [4.91] | | [3.87] | [4.39] | | [.24] | [.24] | | [.25] | [.25] | |
| Misery | −.25 | .81** | .42 | −.31 | 1.00*** | .61 | .50 | .03 | .26 | .36 | .44 | .50 |
| | (−4.39) | (16.54) | | (−4.57) | (16.70) | | (.61) | (.03) | | (.40) | (.50) | |
| | [6.49] | [7.36] | | [4.57] | [5.18] | | [.37] | [.37] | | [.30] | [.30] | |
| Altern. perf. index | .07 | .27 | .10 | .08 | .42 | .23 | .44 | −.31 | .15 | .28 | .16 | .16 |
| | (.89) | (3.97) | | (.72) | (4.12) | | (.39) | (−.28) | | (.19) | (.11) | |
| | [5.90] | [6.70] | | [3.79] | [4.30] | | [.29] | [.29] | | [.23] | [.23] | |
| Avg. 1974–85 – avg. 1963–73 | | | | | | | | | | | | |
| Unemployment | .05 | .19 | .05 | .04 | .21 | .06 | .01 | .10 | .01 | −.22 | .42 | .12 |
| | (.40) | (1.85) | | (.34) | (1.91) | | (.00) | (.06) | | (−.13) | (.26) | |
| | [3.98] | [4.51] | | [3.83] | [4.35] | | [.20] | [.20] | | [.22] | [.22] | |
| Inflation | −.33 | .89** | .45 | −.37 | .97** | .53 | .53* | .09 | .34 | .45 | .32 | .46 |
| | (−3.85) | (11.72) | | (−3.93) | (11.79) | | (.42) | (.07) | | (.37) | (.26) | |
| | [4.08] | [4.63] | | [3.67] | (4.16] | | [.22] | [.23] | | [.23] | [.23] | |
| Misery | −.24 | .83** | .43 | −.29 | .98*** | .61 | .43 | .13 | .25 | .25 | .55* | .52 |
| | (−3.46) | (13.57) | | (−3.60) | (13.70) | | (.42) | (.13) | | (.23) | (.52) | |
| | [5.17] | [5.86] | | [3.81] | [4.32] | | [.30] | [.30] | | [.25] | [.25] | |
| Altern. perf. index | −.06 | .24 | .04 | −.10 | .34 | .07 | .11 | −.12 | .01 | −.21 | .39 | .11 |
| | (−.70) | (2.96) | | (−.83) | (3.08) | | (.08) | (−.09) | | (−.13) | (.24) | |
| | [4.97] | [5.64] | | [3.87] | [4.39] | | [.25] | [.25] | | [.22] | [.22] | |

*Source*: For Cameron's and Schmitter's measurees of centralization and monopoly, see the Appendix, unemployment, misery, and alternative performance index from OECD are reported in Calmfors and Driffill 1988, 20, tbl. 2; inflation is calculated as misery minus unemployment. The alternative performance index is the unemployment rate plus current account deficit in percentage of gross domestic product.

*Note*: Standardized coefficients; unstandardized coefficients are in parentheses, standard errors, in brackets. N for all countries = 14.

*$p \leq .10$, two-tailed test.

**$p \leq .05$, two-tailed test.

***$p \leq .01$, two-tailed test.

by inflating standard errors of regression coefficients (Belsley, Kuh, and Welsch 1980).[16] Serious collinearity can also cause point estimates to take on unexpected signs and to become anomalously large. Commonly used diagnostics (such as Belsley's conditions index) can detect collinearity but do not distinguish its impact on different parameters.

Instead of running diagnostics that would probably have revealed substantial collinearity, we tested the sensitivity of the multiple regression results obtained using Cameron's data to slight changes in collinearity. Technically, this involved multiplying the main diagonal of the moment matrix of the independent variables. A succession of multipliers varying between one and two are used to reduce collinearity among the independent variables. Typically, as the multiplier got large, the centralization effect went to zero, and the monopoly effect shrunk by about a third but remained statistically significant. In other words, the monopoly effect was much less sensitive to collinearity than the centralization effect; the monopoly effect is more robust.[17]

Multivariate analysis across 14 cases using Schmitter's data again produced inconclusive results. Centralization emerged as more important than monopoly in all but one of eight equations. However, only once were results statistically significant. Omitting the Swiss outlier again improves results for monopoly: in five of eight equations, the standardized coefficients for monopoly are larger than for union centralization. Results only attain acceptable levels of statistical significance twice, however, again suggesting that analysis using this data set is not very reliable.

To summarize the results of linear models, bivariate regression analysis, in conjunction with bootstrapping, and multiple regression analysis, in conjunction with sensitivity analysis performed on the stronger results that were obtained using Cameron's data, all tend to confirm that union monopoly is more important than union centralization in accounting for macroeconomic performance. In addition, analyzing Cameron's data shows that monopoly alone generally performs better than his composite index of corporatism. Interpreting the results using Schmitter's data is more difficult, since the analysis often appears statistically unreliable; but with the Swiss outlier omitted, these, too, tend to establish monopoly as more important than union centralization.

## RESULTS OF NONLINEAR MODELS

We now reanalyze the data using nonlinear specifications drawn from Calmfors and Driffill (1988) and Lange and Garrett (1985) to see whether union monopoly continues to emerge as more important than centralization in explaining economic performance. Calmfors and Driffill (1988), in their noted analysis of corporatism and economic performance, argue that corporatism (as well as laissez-faire arrangements characterized by decentralized, enterprise unionism) each breeds good economic performance.[18] The Calmfors–Driffill index of corporatism, comprising a measure of centralization and a measure of monopoly, was devised to test the particular hump-shaped hypothesis that Calmfors and Driffill were pursuing and is slightly different from either Cameron's or Schmitter's indices. Calmfors and Driffill first rank union centralization according to whether wage setting occurs at the enterprise, the industrial, or the confederal level, in order to assess whether wage setting by industrial unions produces results economically inferior to those effected by wage setting by either central confederations or enterprise unions. (See Calmfors and Driffill 1988, 32–33 for a discussion.) They then construct an additive index of corporatism that uses this ranking in conjunction with a ranking of union monopoly, measured by the existence of parallel central organizations and their degree of cooperation.

To test their nonlinear hypothesis, Calmfors and Driffill reorder their overall index of countries according to degrees of corporatism by placing the most and least corporatist ahead of intermediate cases. They then perform rank correlations between this measure and rankings of countries on various measures of economic performance, comparing the results with unrevised indices of corporatism, both their own and Cameron's. In revising their rankings, Calmfors and Driffill offer two alternative specifications. The first (ranking A) puts the most corporatist economy first, followed by the least corporatist, followed by the second most, then the second least, and so forth. The second (ranking B) puts the three most corporatist first, followed by the three least corporatist, followed by the second three most corporatist, and so forth. To test the explanatory strength of union monopoly against Calmfors and Driffill's hump-shaped interpretation of corporatism, I performed bivariate regressions against the eight measures of the dependent variable with which we are familiar. As independent variables, I used Cameron's and Schmitter's measures of union monopoly and Calmfors and Driffill's revised rankings of corporatism. Corroboration of the monopoly hypothesis requires that the equations based on Cameron's and Schmitter's measures of monopoly perform better than those based on Calmfors and Driffill's two indices of corporatism.[19]

Eight of the sixteen bivariate regressions using the Calmfors–Driffill revised indices obtained acceptable levels of statistical significance. A comparison of the performance of Calmfors and Driffill's two revised corporatist indices against Cameron's and Schmitter's measures of monopoly reveals that Cameron's indicator for monopoly outperforms either of Calmfors and Driffill's rankings of corporatism in four of eight cases, and that their ranking B obtained higher r-squareds than both Cameron's and Schmitter's measures of monopoly in the other four equations.[20] Thus, composite indices of corporatism are not clearly superior to simple measures of union monopoly. Nor is the nonlinear specification proposed by Calmfors and Driffill obviously superior to a linear specification that uses union monopoly, rather than a composite measure of corporatism.

Lange and Garrett (1985), in an argument with implications similar to those advanced by Calmfors and Driffill, argue that both encompassing trade unionism and left-wing control of government are required for improved economic performance, proposing as well that where both of these are absent, improved economic performance may also result.[21] Although hypothesizing a nonlinear relationship between union structure and macroeconomic performance similar to Calmfors and Driffill's, the analytic techniques differ. Lange and Garrett use multiple regression analysis in which corporatism, left-wing political power, and the multiplicative combination of these two variables are regressed against measures of economic performance.[22] For independent variables in their most successful regressions, Lange and Garrett use (1) a composite index of the organizational power of labor, taking measures drawn from Cameron,[23] and (2) a measure of left-wing political power, namely, the mean percentage of cabinet portfolios held by leftist parties between 1974 and 1980 (Lange and Garrett 1985, 805, tbl. 2). They perform their analysis using 15 observations—the 14 I just used, minus Switzerland but including Japan and Australia.

To assess whether the performance of the Lange–Garrett model would improve with a simple measure of union monopoly rather than a measure of centralization or an index of corporatism, I replicated the Lange–Garrett equations using first a measure of monopoly, then a measure of centralization, and finally, an index of corporatism.[24] Recall that Lange and Garrett's original equations comprised three terms for the independent variables: corporatism, leftwing political power, and a multiplicative combination of the two first terms. For the first term, I sequentially substituted Cameron's and then Schmitter's measure of monopoly, then each of their measures of centralization, and finally each of their measures of corporatism; in each equation, the third, multiplicative, term was formed accordingly. Analysis thereby generated two sets of 48 equations, one using both Schmitter's and Cameron's data across 13 observations (the same data set as before, minus Switzerland), the other using only Cameron's data across 15 observations (i.e., including Japan and Australia).

In the analysis across 13 cases for Cameron's and Schmitter's data, of the 48 equations, only 18 produced statistically significant results, indicating that the analysis is not very reliable. Corporatism and monopoly were statistically significant five and six times, respectively. Comparing pairs of equations (i.e., regressions using identical dependent variables but substituting monopoly for centralization), those using monopoly generated higher r-squareds than those using centralization in six of eight equations using Cameron's data set. In the regressions across 15 observations also using that data set, six of the eight pairs of equations featuring union monopoly generated higher r-squareds than those testing union centralization. Only seven of 24 equations produced statistically significant results, however. Thus, results largely favor the monopoly hypothesis but are not statistically conclusive. Results from Schmitter's data failed to corroborate the hypothesis: only once did monopoly generate a higher coefficient than centralization, although half of the regressions obtained acceptable levels of statistical significance.

Comparing the importance of all three independent variables (corporatism, centralization, and monopoly) across 13 observations finds that corporatism obtained the highest r-squared three times compared with the five times monopoly achieved the highest r-squared using Cameron's data. Thus, results reconfirm that a simple measure of monopoly outperforms Cameron's composite index of corporatism. Using Schmitter's generated different results, however. In this case, centralization obtained best results in six of eight groups of equations; and corporatism obtained best results in the other two. While these results fail to establish the importance of union monopoly (instead, highlighting centralization), they, too, cast doubt on the explanatory power of composite corporatist indices.

Reanalyzing nonlinear models proposed by both Calmfors and Driffill (1988) and Lange and Garrett (1985) thus lends additional, albeit not very statistically compelling, support to the hypothesis that union monopoly is a better predictor of variations in macroeconomic performance than is union centralization. This emerges far more strongly with Cameron's data set than with Schmitter's. Results with both data sets also throw doubt on the composite corporatist index, which proved less powerful statistically than a measure of union monopoly or, in Schmitter's case, centralization. Finally, using a linear specification of monopoly against Calmfors and Driffill's nonlinear corporatist index reestablishes the linear model as competitive with its nonlinear challenger.

## THE COORDINATION OF WAGE SETTING VERSUS UNION MONOPOLY

Much in the spirit of the present analysis, a recent paper by Soskice (1990) rejects the parabolic hypotheses advanced by both Calmfors and Driffill (1988) and Lange and Garrett (1985), attempting to rehabilitate a linear model. Soskice contends that linear models have suffered in comparison with nonlinear ones because of the misclassification of cases, including some statistically crucial ones. On the basis of a meticulous, case-by-case analysis of 11 countries, he offers an alternative classification for what he calls the economywide coordination (EWC) of wage setting.

Soskice reclassifies countries as "coordinated" if they enjoy either highly formal, centralized collective-bargaining institutions or if their collective-bargaining processes are informally well coordinated. The latter, he argues, is especially likely to occur where employers are highly centralized even if unions are not. He then seeks to account for the alternative measure of economic performance used earlier (the unemployment rate plus the current account deficit as percentage of gross domestic product) for the period 1985–89 using EWC in conjunction with a measure of local wage "pushfulness." The latter refers to the degree to which workplaces enjoy the capacity to undertake autonomous collective action in the wage-setting process in conjunction with whether the incentive structure facing workers is likely to lead them to do so. Even if collective bargaining is highly coordinated, Soskice hypothesizes, such coordination may be disrupted if groups of workers have both the incentive and the capacity to undertake wage bargaining on their own.

To assess the strength of union monopoly against Soskice's measure of EWC, I performed regressions that replicated his for the standard time periods used in the present analysis. I compared the results of multiple regressions that substituted Cameron's and Schmitter's measures of union monopoly in place of EWC.[25] Results are presented in Table 3. The regressions generally produce good r-squareds and the signs are in the expected directions. Union monopoly and EWC are positively associated with measures of macroeconomic performance, whereas the impact of local "pushfulness" is negative. Soskice's EWC performs quite well—indeed, about as well as union monopoly. Inspecting the results of eights sets of equations, those using EWC generated higher r-squareds than those using measures of union monopoly in half the cases. Acceptable levels of statistical significance are achieved equally by EWC and monopoly measures.[26]

Soskice's analysis of collective-bargaining structures thus emerges as the strongest contender against

**TABLE 3**

**Multiple Regression Models Comparing Economy wide Coordination (EWC) with Measures of Union Monopoly**

| | CAMERON'S MEASURE | | | SCHMITTER'S MEASURE | | | SOSKICE'S MEASURE | | |
|---|---|---|---|---|---|---|---|---|---|
| DEPENDENT VARIBLE | MONOPOLY | "PUSHFULNESS" | $r^2$ | MONOPOLY | "PUSHFULNESS" | $r^2$ | EWC | "PUSHFULNESS" | $r^2$ |
| Avg. level 1974–85 | | | | | | | | | |
| Unemployment | .58* | −.28 | .54 | .33 | −.42 | .34 | .71** | −.20 | .66 |
| | (6.23) | (−.92) | | (.32) | (−1.38) | | (1.18) | (−.67) | |
| | [2.88] | [.89] | | [.30] | [1.03] | | [.39] | [.78] | |
| Inflation | .48 | −.46 | .60 | .21 | −.59* | .44 | .31 | −.51 | .48 |
| | (6.43) | (−1.93) | | (.26) | (−2.46) | | (.65) | (−2.13) | |
| | [3.46] | [1.07] | | [.36] | [1.21] | | [.63] | [1.25] | |
| Misery | .61** | −.44* | .75 | .30 | −.59* | .51 | .56** | −.43 | .69 |
| | (12.67) | (−2.85) | | (.58) | (−3.84) | | (1.82) | (−2.80) | |
| | [4.25] | [1.32] | | [.52] | [1.74] | | [.75] | [1.50] | |
| Altern. perf. index | .36 | −.45 | .45 | −.05 | −.59 | .34 | .52* | −.37 | .56 |
| | (4.56) | (−1.76) | | (−.06) | (−2.30) | | (1.02) | (−1.45) | |
| | [3.76] | [1.16] | | [.36] | [1.22] | | [.53] | [1.06] | |
| Avg. 1974–85 − avg. 1963–73 | | | | | | | | | |
| Unemployment | .32 | −.39 | .34 | .26 | −.45 | .32 | .44 | −.33 | .41 |
| | (2.69) | (−1.01) | | (.20) | (−1.17) | | (.57) | (−.85) | |
| | [2.74] | [.85] | | [.25] | [.83] | | [.41] | [.82] | |
| Inflation | .54* | −.34 | .53 | .28 | −.47 | .36 | .33 | −.40 | .37 |
| | (7.64) | (−1.51) | | (.37) | (−2.10) | | (.73) | (−1.77) | |
| | [3.92] | [1.22] | | [.40] | [1.36] | | [.73] | [1.44] | |
| Misery | .61** | −.48** | .81 | .36 | −.62** | .61 | .49* | −.50* | .68 |
| | (10.33) | (−2.52) | | (.57) | (−3.27) | | (1.30) | (−2.63) | |
| | [3.00] | [.93] | | [.37] | [1.27] | | [.61] | [1.21] | |
| Altern. perf. index | .24 | −.56 | .46 | −.03 | −.64* | .41 | .33 | −.51 | .50 |
| | (2.18) | (−1.58) | | (−.02) | (−1.83) | | (.48) | (−.14) | |
| | [2.72] | [.84] | | [.25] | [.84] | | [.42] | [.82] | |

*Sources*: For Cameron's and Schmitter's measures of monopoly, see the Appendix; Soskice's measures of economywide coordination and local pushfulness from Soskice 1990, tbls. 1–2; unemployment, misery, and alternative performance index from OECD are reported in Calmfors and Driffill 1988, 20, tbl. 2; inflation is calculated as misery minus unemployment. The alternative performance index is the unemployment rate plus current acoount deficit in percentage of gross domestic product.

*Note*: Standardized coefficents; unstandardized coefficients are in parentheses, standard errors, in brackets. N = 10.

*$p \le .10$, two-tailed test.

**$p \le .05$, two-tailed test.

the hypothesis of union monopoly in assessing the impact of industrial relations on macroeconomic performance. One advantage offered by an account using union monopoly over Soskice's concept of bargaining coordination lies with measurement simplicity, however. Soskice's reclassification of countries uses three separate indicators for bargaining coordination: formal union centralization in Austria, Norway, and Sweden; powerfully coordinated employers' organizations in Japan and Switzerland; and de facto bargaining coordination across unions in Germany and the Netherlands (pp. 41–42, 44). This reflects a keen sensitivity to national bargaining conditions; but the concept of union monopoly uses simpler, identical indicators across countries and so may be preferable.

More importantly, perhaps, the present analysis has tried to show that there may be compelling conceptual and empirical reasons to distinguish the effects of union centralization from monopoly. Like composite measures of corporatism, Soskice's economywide coordination deliberately combines the two, with the rationale that they are functionally equivalent. While his recoding of countries corrects some measurement problems, especially for centralization, the results presented here question the assumption of functional equivalence between union monopoly and centralization.

## CONCLUSIONS

The results of the statistical analyses I have reported are necessarily modest, given the small number of cases available for analysis and such problems as relatively high multicollinearity among independent variables. Results are often not very robust, and small perturbations in measurement or in the data set used may lead to different (or at least inconclusive) results. Nonetheless, the pattern that emerges across numerous empirical tests corroborates findings reported but not analyzed in earlier studies showing that union monopoly is more important than union centralization in accounting for comparative macroeconomic performance in the 1970s and 1980s. These results cast doubt on the argument that the formal centralization of authority in the labor movement is a prerequisite for an effective incomes policy, for sustained wage moderation, or for the better economic performance that should follow. The binding authority that central confederations appear to hold is suspect: ultimately, it rests on the voluntary acquiescence of union affiliates. For this reason, the number of national unions and central confederations is a more important structural characteristic of organized labor affecting national economic performance. Additionally, since at least as good statistical results can be obtained from a simple measure of union monopoly as from composite measures of corporatism, composite measures should be reconsidered. Union monopoly is an economical, easily quantifiable, and surprisingly powerful determinant of national economic performance.

The findings reported here suggest a research agenda for the future. First, arguably, much of the recent cross-national research on corporatism should be redone but with significantly greater care in constructing the independent variables. Second (relatedly), the research reported here directs greater attention to trade unionism and collective bargaining, areas of inquiry that political scientists have tended to leave to industrial relations specialists and economists. To the extent that an understanding of the impact of organized labor on the political economy hinges on a proper assessment of labor as an organizational actor, political scientists would do well to reconsider the importance of organized labor and to turn more of their attention to the dynamics of trade unionism.

## APPENDIX: INDICATORS AND MEASUREMENT OF THE INDEPENDENT VARIABLES

Cameron and Schmitter each based his measure of union centralization on an earlier study by Headey (1970). Headey, in turn, had used a fourfold index to rank unions by their degree of centralization. This index comprised (1) the confederal role in collective bargaining (particularly regarding wage demands); (2) confederal power over strike action; (3) the size of confederal staff relative to membership; and (4) the proportion of membership dues accruing to the confederation.[27] This index of centralization has since become more or less standard, having been taken up (with amendments) not only by both Schmitter (1981) and Cameron (1984), but then, also, by those working with third-generation indices drawing on Schmitter and Cameron (Alvarez, Garrett, and Lange 1991; Czada 1987; Garrett and Lange 1986; Hicks 1988; Lange and Garrett 1985; Marks 1986; Wallerstein 1990).

Although based on the same source, Cameron's and Schmitter's indices of centralization are not identical. Schmitter's index was lifted unaltered from Headey, and their rank-orderings of countries are therefore indistinguishable. Cameron, however, modified Headey's original index, devising a fourfold index of what he calls *"confederation power in collective bargaining,"* comprising whether the confederation (1) consults with unions about wage negotiations prior to collective bargaining, (2) itself participates in collective bargaining, (3) possesses the right to veto negotiated settlements, and (4) controls the distribution of strike funds to unions (1984, 164). The modified index partially obviates some validity problems found in Headey's original index and may be preferred for that reason (see my n. 27).

**TABLE A-1**

**Cameron's and Schmitter's Measures of Trade Union Centralization, Monopoly, and Corporatism**

| | CAMERON'S MEASURES | | | | SCHMITTER'S MEASURES | | |
|---|---|---|---|---|---|---|---|
| OBSERVATION | CENTRALIZ. | MONOP. | DENSITY | CORPORAT. | CENTRALIZ. | MONOP. | CORPORAT. |
| Austria | .8 | 1.0 | 50 | 90 | 1 | 3 | 1 |
| Belgium | .6 | .6 | 55 | 66 | 3 | 8.5 | 7 |
| Britain | .3 | .4 | 45 | 31.5 | 12.5 | 11 | 13 |
| Canada | 0 | .4 | 27 | 10.8 | 12.5 | 8.5 | 10.5 |
| Denmark | .4 | .8 | 54 | 64.8 | 8 | 1.5 | 4 |
| Finland | .6 | .8 | 47 | 65.8 | 5 | 4.5 | 4 |
| France | 0 | .2 | 24 | 4.8 | 10 | 13 | 12 |
| Germany | .2 | .8 | 32 | 32 | 9 | 6 | 8 |
| Italy | .2 | .2 | 41 | 16.4 | 12.5 | 13 | 14 |
| Netherlands | .6 | .6 | 28 | 33.6 | 2 | 8.5 | 6 |
| Norway | .7 | .8 | 65 | 97.5 | 5 | 1.5 | 2 |
| Sweden | .7 | .8 | 70 | 105 | 5 | 4.5 | 4 |
| Switzerland | .4 | .6 | 24 | 24 | 7 | 13 | 9 |
| United States | 0 | .4 | 21 | 8.4 | 12.5 | 8.5 | 10.5 |
| Japan | .1 | .2 | 16 | 4.8 | — | — | — |
| Australia | .3 | .4 | 40 | 28 | — | — | — |

*Sources*: Cameron 1984, 165, tbl. 7.6; Schmitter 1981, 294, tbl. 10.1.

*Notes*: In Cameron, centralization is labeled "confederation power in collective bargaining," monopoly is labeled "organizational unity of labour," density is "average % of total labour force unionized," and corporatism is centralization plus monopoly times density. In Schmitter, centralization is labeled "organizational centralization," monopoly is labeled "associational monopoly," and corporatism is labeled "societal corporatism." Countries not included on the dependent variable (Calmfors and Driffill 1988) are omitted.

A measure of union monopoly was first introduced by Schmitter (1981), using data from the *Europa Yearbook.* An additive index, Schmitter's measure involves (1) the number (from one to three) of national confederations in existence, (2) whether manual and nonmanual employees are organized in different unions, and (3) whether national unions and/or confederations house stable factions (p. 294, tbl. 10.1). Cameron, too, proposes a measure of what he called the organizational unity of labor, also drawing on data from the *Europa Yearbook.* His measure comprises the number of confederations and the number of unions affiliated with them (1984, 164).

Both Cameron and Schmitter construct indices of corporatism. Schmitter's is an additive combination of his ranking of countries according to their degree of union centralization and monopoly. Cameron's index, labeled "the organizational power of labor" in a deliberate attempt to distinguish the structural aspects of trade unionism from corporatism as a political phenomenon, also incorporates the level of unionization, or union density. His measure (I refer to it as an *index of corporatism*) comprises his rankings of union concentration plus centralization times the average level of unionization. Table A-1 presents the coding of countries done by Cameron and Schmitter.

## Notes

Earlier versions of this research were reported at the Wissenschaftszentrum, Berlin, 1991; at a conference, The New Institutionalism: State, Society and Economy, at the University of Colorado, Boulder, 1990; and at the meetings of the American Political Science Association, Atlanta, 1989. For comments, I thank David Cameron, Geoffrey Garrett, Peter Lange, Margaret Levi, Jeannette Money, Jonas Pontusson, David Soskice, Peter Swenson, Sidney Tarrow, Kathleen Thelen, and Michael Wallerstein. Advice on data analysis was provided by Stephen Ansolabehere, Barbara Geddes, John Zaller, and especially George Tsebelis. Bruce Western did some excellent statistical work for the project. Financial support was provided by the German Marshall Fund of the United States and, at the University of California, Los Angeles, by the Academic Senate, the Institute of Industrial Relations, the International Studies and Overseas Programs, and the Center for International Business Education and Research.

1. Exceptions come mainly from the field of economics, which has been more attentive to investigating micro conditions for wage restraint and lower unemployment (e.g. Bean, Layard, and Nickell 1986; Calmfors and Driffill 1988; see also Moene, Wallerstein, and Hoel 1993).

2. One standard account of how employers succeeded in breaking up centralization and striking a separate agreement with the metalworkers union in 1983 illustrates this when it reports that "the immediate explanation given by a national union representative, in his account of why *Metall* had not more strongly opposed the 1983 rift [was that] 'We were losing too many members to SIF [the white-collar engineering union]' " (Lash 1985, 222).

3. Annual German unemployment for the years 1974–85 averaged 4.8%, while inflation rates averaged only 4.4%. Both compare favorably with an annual average of 5.4% unemployment and, particularly, with 9% inflation for 17 advanced capitalist economies during the same decade (calculated using OECD data reported in Calmfors and Driffill 1988, 20, tbl. 2).

4. The same argument can be made for density. The two Scandinavian countries that experienced collapses in union centralization in the 1980s were Denmark and Sweden, which were also the two where unionization rates underwent greatest increase (see Golden, Lange, and Wallerstein 1992, figs. 1, 3, 7).

5. Occasionally, confederations are comprised of workers, not of other unions; that is, employees join the confederation directly and are subsequently grouped into the appropriate industrial union. This arrangement is relatively rare, and almost all confederations are technically peak associations (organizations of other organizations).

6. Density ignores the impact of extension laws, which legally extend collectively bargained wage rates to nonunion members and/or firms not affiliated with the contracting employers' organizations. Where extension laws obtain, density is a poor measure of the encompassingness of union collective bargaining. To the best of my knowledge, systematic data on extension laws in OECD countries have not been gathered; but they are important in many continental European countries. For an eventual source of data, consult Golden, Lange, and Wallerstein n.d.

7. Calmfors and Driffill (1988) also provide separate measures of centralization and monopoly; but their scale ranges only from zero to three, thereby failing to provide enough variation to generate useful statistical results.

8. The measures are standardized; and Cameron's is inverted to run in the same direction as Schmitter's (from most to least, rather than the reverse).

9. The standardized correlation coefficients between Cameron's measure of density and Schmitter's measure of centralization is .45, whereas between density and monopoly, it is .65.

10. For these reasons, I choose not to use either wages or economic growth as dependent variables. Specifically, I did not use a measure of wages for two reasons. First, changes in wages per se are not usually regarded as measuring changes in macroeconomic performance. At the aggregate level, wages are important because they have an impact on the inflation rate, for instance. Since inflation rates also capture how far changes in wages are passed on to prices, it is a more general measure of the impact of wage bargaining on the economy as a whole. Second, to the best of my knowledge, there is no standard, generally accepted data set available on wages for the years and countries required.

As for economic growth, analysis with gross domestic product growth as the dependent variable produced results statistically too weak to be reliable. I suspect this is because growth is much more removed from collective bargaining than such phenomena as inflation; that is, there are too many other unmeasured variables affecting growth. Since the purpose of the present study is to investigate competing hypotheses about trade unionism, not to explain macroeconomic performance as such, it seemed appropriate to develop the analysis for indicators that performed adequately and to omit those that did not.

11. Calmfors and Driffill include Australia, New Zealand, and Japan, none of which is included in Schmitter's data set. (Cameron includes Australia and Japan, as well as Spain.) At the same time, Cameron and Schmitter both include measures on the Republic of Ireland, which does not appear in Calmfors and Driffill's study. Investigation of OECD sources showed that complete data for Ireland do not appear to be available. All told, then, data for 14 countries were available on both the independent and dependent variables. Later parts of the analysis drop Schmitter's data set in order to include Japan and Australia.

12. Correlations between the average level for the years 1974–85 and the change from 1963–73 are as follows: inflation $r = .93$, unemployment $r = .84$, combined misery level $r = .98$, alternative performance measure $r = .90$.

13. Where statistical results are only summarized in the text, complete analyses are available on request.

14. These findings corroborate results reported, but not commented on, by Cameron himself (1984, 169, tbl. 7.7). His own analysis showed that union concentration was consistently more strongly associated with various measures of economic performance (including unemployment, inflation, strike activity, and wage increases) than were other attributes of union structure, including centralization or the overall organizational power of labor (Cameron's term for corporatism). If we transform the coefficients he reports into r-squareds, we find that for the period 1965–82, Cameron found $r^2 = .27$, regressing union monopoly against unemployment rates for 18 countries; $r^2 =$ only .19, regressing

union centralization against the same dependent variable; and $r^2$ = only .16, for corporatism.

15. This part of the analysis was undertaken by Bruce Western.

16. The analysis reported in this section was undertaken by Bruce Western.

17. Similar sensitivity analyses have been proposed in a Bayesian setting by Leamer (1978).

18. Among their dependent variables, Calmfors and Driffill include a measure of employment (which I omit) and did not consider a measure of inflation, as I do. Their other measures of the dependent variable are the same as those used here. My periodization, taken directly from them, is, of course, also identical. Calmfors and Driffill test their hypothesis using 17 observations. Reassessing their hypothesis using both Cameron's and Schmitter's data sets requires reducing the number to 14.

19. I also tried reranking Cameron's and Schmitter's measures of monopoly and centralization using the schemes proposed by Calmfors and Driffill and regressing these against the dependent variable. However, it proved difficult to reorder Cameron's and Schmitter's measures using Calmfors and Driffill's schemes A and B, largely because so many countries are coded identically in each of the two data sets that reranking them without ties called for considerable personal judgment. Perhaps because of this, the results of the bivariate regressions were unimpressive. R-squareds were generally extremely low (in all but four of 32 cases, below .10); and only two equations produced statistically significant results. Had the reranking been successful, comparing regression results for these reordered measures of union centralization and monopoly would have tested whether the monopoly hypothesis held even under nonlinear conditions assumed by Calmfors and Driffill.

20. To assess how results would hold with the inclusion of Japan (an important case for Calmfors and Driffill's results) or whether the results were, instead, an artifact of the number of observations, I also analyzed Calmfors and Driffill's revised rankings and Cameron's measure of monopoly including Australia and Japan. Results were again mixed: the coefficients were higher for Calmfors and Driffill's revised ranking B in five cases, higher for Cameron's measure of monopoly in two, and about equal in one.

21. Later papers, including Garrett and Lange 1986 and Alvarez, Garrett, and Lange 1991, although statistically more complex than their original 1985 presentation, do not substantially alter their model. Moreover, all these papers use the same measures of union centralization as those in the first piece of research. Thus, I take their original and simplest formulation.

22. In their first analysis, their measure of the dependent variable was confined solely to growth rates; for a discussion, see n. 10, above.

23. Lange and Garrett do not use Cameron's measure of the organizational power of labor (monopoly plus centralization times density) as their indicator of corporatism. Instead, using Cameron's data, Lange and Garrett sum monopoly, centralization, and a third measure—the scope of collective bargaining—and add this index (which they label *centralization*) to Cameron's measure of union density. They provide no justification for altering the composition of the independent variables taken from Cameron.

24. Thus, in this case, we are able to assess the importance of monopoly within a nonlinear model, as was not the case with the Calmfors–Driffill hypothesis.

25. Soskice's analysis includes Austria, Britain, France, Germany, Italy, the Netherlands, Norway, Sweden, Switzerland, the United States, and Japan. To compare his hypothesis against the monopoly hypothesis using both Cameron's and Schmitter's data, I dropped Japan (which Schmitter did not include). Switzerland could not be omitted (as it was in earlier linear analyses) without losing too many degrees of freedom.

26. A similar analysis comparing Soskice's EWC and local "pushfulness" with Cameron's measure of monopoly and local "pushfulness," with Japan included (a case Schmitter fails to code) produced similar results. EWC and monopoly performed equally well, each generating higher r-squareds half the time. Ten of the 16 equations produced statistically significant results.

27. Although it has been accepted more or less uncritically, Headey's index of centralization exhibits validity problems. Two of his four criteria conflate general union weakness and a lack of confederal authority. An illustration is provided by the Italian case, which Headey ranks as the most decentralized union movement among those examined. He notes that none of the Italian confederations control strike funds and that their staffs are small relative to membership. But there are no strike funds to control in Italy; and while confederal staff may be small relative to membership, the relevant issue is, instead, the size of confederal staff relative to overall union staff. Published in 1970, Headey's study presumably reflects the situation in the 1950s and 1960s. For this period, his ranking is inaccurate. In the 1950s and for much of the 1960s, what effective control over strikes Italian unions exercised (not a great deal) lay largely with the central confederations; similarly, if staff size was generally small, it would have been relatively larger for the confederations than for national unions, at least until the mid-1960s. The Italian should thus be considered highly centralized—among the most centralized for the 1950s and only somewhat less for most of the 1960s, as is properly noted by Windmuller (1975, 100, n. 60).

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# PREDICTING THE NUMBER OF PARTIES: A QUANTITATIVE MODEL OF DUVERGER'S MECHANICAL EFFECT

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The mechanical effect *of electoral systems, identified by Maurice Duverger, can be estimated by means of a quantitative model. The model predicts the range within which the* effective number *of parties in a district should fall for a given magnitude (number of seats) of the district. At the national level, a related model predicts the effective number of parties based on the effective magnitude and the number of seats in the national assembly. The institutional variables considered—magnitude and assembly size—define a great portion of the structural constraints within which a given country's politics must take place. The model developed provides a good fit to data in spite of its having been developed from outrageously simple starting assumptions.*

If one had to give a single number to characterize the politics of any country that employs competitive elections, it would be the number of parties active in its national assembly. This number would not tell the whole story, by any means; but it tells us more than any other single number or term could. The number of parties directly or indirectly affects other important aspects of how a political system functions, including how long its cabinets last if the system is parliamentary (Lijphart 1984) and how elections translate into "citizen control" of policymakers (Powell 1989). The number of parties is a most important feature in a country's politics and therefore in comparative studies also. We shall discuss the ways of defining this number operationally further on.

What determines the number of parties? History, present issues, and institutions all intervene. But if one had to give a single major factor, it would have to be the *district magnitude* (M), that is, the number of seats allocated in an electoral district (Rae 1967). The well-known Duverger rule says that one-seat districts tend to lead to two-party systems, while multiseat districts tend to go with multiparty systems (Duverger 1951, 1954; see also Riker 1982). One can be more precise, since even within the multiseat category, a larger M tends to go with a larger number of parties. But before we can attempt to define a relationship between district magnitude and the number of parties, we need to establish a means to measure the number of parties.

## How To Measure the Number of Parties

In this work, we shall use the *effective number* of parties, rather than the actual number. This measure is defined as follows:

$$N = (\Sigma p_i^2)^{-1}, \qquad (1)$$

where $p_i$ is the share of votes or seats won by the *i*th party (Laakso and Taagepera 1979). The effective number tells us the number of hypothetical equal-sized parties that would have the same effect on the fractionalization of the party system as have the actual varying sized parties.[1] The advantage of using the effective, rather than the actual, number of parties (or actual number of parties above some arbitrary cutoff) is that it establishes a nonarbitrary way to distinguish "significant" parties from less significant ones. The construction of the index is such that each party weights itself by being squared. Tiny parties contribute little to the index, while large parties contribute relatively more.[2]

## The Empirical Relationship and Methods of Estimation

The average empirical relationship is expressed by "the generalized Duverger's rule":

$$N_s = 1.15(2 + \log M), \qquad (2)$$

where logM is the decimal logarithm of the district magnitude (see our earlier work, Taagepera and Shugart 1989, 142–55). Such empirical relationships are useful but not sufficient. We shall seek a quantitative model to explain why the average number of parties at a given M tends to be precisely that large and not much smaller or larger. Such a model would capture the outcome of how a nation's politics are filtered through one very important political institution, namely, the number of seats for which politicians compete. Qualitatively, this process is the Duverger *mechanical effect* of district magnitude. With M = 1, only one party can win; and this has a strong tendency to impel the political forces to conglomerate into two large parties in order to have some chance of winning. As M increases, more parties can possibly win seats in the given district; hence, more parties can afford to run.

In building our model, we shall seek an explanation grounded in something more firm than simply an observation that such and such an equation is the best fit to the data. We seek to find a starting point—or set of starting points—that possess a certain deductive elegance. We do so by taking a variable whose upper and lower extreme possible values are easily established and by assuming that the average value between the extremes will prevail. Upon confronting such assumptions with data, the original estimates can be corrected if necessary. We prefer to make our corrections not through an "error" term or by expanding from bivariate to multivariate analysis but by going back to the original assumption and considering factors that might perturb it and lead to a different estimate. Thus, we use a mix of alternating deductive and inductive approaches, always trying to find the simplest possible model that appears to gain explanatory leverage. Our method is rather unconventional in the social sciences but is typical of disciplines as diverse as physics and sabermetrics (baseball analysis).[3] Our method appears to be especially appropriate for fields in which it is possible to define clear upper and lower limits to the value that certain variables can take and also, of course, where variables already come in ready quantitative form (e.g., parties and seats, mass and energy, runs and wins). The method, as applied to electoral analysis, thus produces results that many may consider mechanical, with no specification of actual social processes included. Indeed, far from being a weakness of our method, such a feature is its principal strength.

We are, after all, seeking to quantify the mechanical effect of the political institution called district magnitude; therefore, one should not be surprised to find that the enterprise itself turns out to be mechanical. It is mechanical in the sense of accounting not for political issues, personalities, or culture but only for the limits imposed on such features by institutional structures. Institutions shape what politicians prefer and, even more, how they pursue their preferences. Our dependent variable, the number of parties, shows the outcome of the aggregation of politicians' pursuits. Furthermore, our theory is limited to the number of parties in the national assembly, as distinct from parties participating in elections. The Duverger mechanical effect deals only with the parliamentary parties. The indirect impact on electoral parties—the Duverger "psychological effect," as discussed by Reed (1990)—is outside our scope here.

Proceeding from the simple and basic to the more complex, we shall first consider the seats parties win in a single electoral district. We can then proceed to the nationwide combination of the district-level effects, also keeping in mind the supradistrict effects of adjustment seats and thresholds. District-level effects of electoral rules have largely been neglected. (Major exceptions are Cox 1990; Katz 1980; Reed 1990; and Sartori 1986). Yet one cannot hope to explain the more complex nationwide phenomena without first elucidating the relatively simple scene in a single district.

## THE EFFECTIVE NUMBER OF SEAT-WINNING PARTIES IN A DISTRICT

It is now time to begin building a model of the effective number of seat-winning parties in a district, which we shall designate $N'_s$. (Symbols without the prime will indicate the corresponding quantity at the nationwide level.) In building such a model, we rely on three basic equations, defining relations among several variables and then combining these equations into one that expresses the effective number of seat-winning parties in a district in terms of the magnitude of the district. The steps are

1. Estimate $N'_s$ in terms of the seat share of the largest party.
2. Estimate the actual (not effective) number of parties winning seats in a district.
3. Estimate the relationship between the actual number of seat-winning parties and the seat share of the largest of these parties.

The logic behind each of these steps is simple. The end result is to get an estimate of $N'_s$ in terms of M. We know that $N'_s$ is (by definition) an estimate of how many "significant" parties there are in a given distribution of seats, so the most useful way to estimate $N'_s$ in terms of a single observable (or estimatable) variable is to do so on the basis of the seat share of the most "significant" of all the parties—the largest one. This is step 1. Step 2 is independent of the first and allows us to get closer to an estimate of the seat share of the largest party, which we need if step 1 is to lead us to our goal. Step 2 is necessary because, while we could not easily arrive at a deductive estimate for the *effective* number of parties in a district, for the *actual* number of parties, such an estimate is simple (as we shall see). Having that estimate, then, lets us get an estimate for the seat share of the largest party (step 3). The steps can then be algebraically brought together to give us an estimate of $N'_s$ in terms of M, for which we shall provide a graphical test. In order to keep the exposition accessible to a wider political science audience, not all the details of these building-block steps will be shown.

### Step 1. Relation of the Effective Number of Seat-winning Parties to the Seat Share of the Largest Party

The value of $N'_s$ is to an appreciable extent determined by the share of the largest component. When the other parties are extremely splintered, $N'_s = 1/s_1'^2$. When all the other parties are as large as the largest party, $N'_s = 1/s'_1$. Let us test for the possibility that the actual values will be around the geometrical mean of these extremes:[4]

$$N'_s = s_1'^{-1.5}. \tag{3}$$

A graph of this relationship (not shown) gave satisfactory agreement with data but revealed that a best-fitting line would have given an estimate of the values of $N'_s$ about 30% lower. The test of whether this is adequate will come with the confrontation of the final model with data. But first, we go to step 2.

### Step 2. Relation Between District Magnitude and the Actual Number of Seat-winning Parties in a District

In a district of M seats, the minimum number of parties is 1 (when one party wins all the seats) and the maximum is M (when each seat is won by a different party). We expect there to be an average number of actual seat-winning parties lying somewhere between 1 and M, so let us estimate the actual number of seat-winning parties in a district, p′, using the geometrical mean:

$$p' = M^{.5} \qquad \textbf{(4)}$$

This estimate produced a reasonably good fit but could have been better. A correction can be made, based upon the notion that a given district usually does not stand alone; rather, it is one of several districts from which the members of the larger parliament are elected.

*Correcting for National Politics.* The model can be improved by observing that there is a difference between a district of $M = 20$ within a larger country and a small country with an assembly of 20 seats, all elected within a single nationwide district. In the latter case, all parties are generated within the district. In the former case, some nationwide parties with no ready constituency within that particular district may still bring in funds and talent from the outside and possibly win a seat. It is the difference between a closed system (where the model of equation 4 may apply) and a more complex, open system. Our correction factor brings in the number of seats (S) in the national assembly.

The effect of assembly size S must enter in the form of the ratio S/M, since it must vanish when the entire country is made one single district ($M = S$). To satisfy the conditions that $M > p' > M^{.5}$ at any district magnitude, and $p' = M^{.5}$ at $M = S$, the expression for p′ must have the form

$$p' = M^k, \qquad \textbf{(5)}$$

where $k = 1/[1 + (M/S)^n]$, n being a constant, as yet unspecified. Furthermore, p′ at any $M < S$ must not be higher than it is at $M = S$. This condition would be violated, if we set $n = 1$. The largest acceptable value of n would be one that makes p′ almost reach the value of p′(S) at large $M < S$; that means a value of n that makes the slope of the curve zero ($dp'/dM = 0$) for $M = S$. Taking the derivative of p′ in the equation above and setting it to zero at $M = S$ yields $n = 2/\ln S$. This is the maximum value n could take, corresponding to the maximum possible effect of nationwide politics on the given district. The minimum possible effect is, of course, no effect, which corresponds to $n = 0$ (and thus $p' = M^{.5}$ at any M). In the absence of any further information, the expectation value of n is taken as the mean of the extremes, that is, $n = 1/\ln S$. Now equation 6 becomes

$$k = 1/[1 + (M/S)^{1/\ln S}]. \qquad \textbf{(6)}$$

For a single nationwide district ($M = S$), this reduces itself to $k = .5$ and thus equation 2. For $M < 30$, the value of p′ is very little affected by the precise value of S, as long as it is larger than 60. The median assembly size for independent countries is around 150, and the usual range is 50–500. The value $S = 148$ corresponds to the round figure of $1/\ln S = .200$. Because $1/150^{.200} = .37$, the result is

$$k = 1/[1 + .37M^{.2}]. \qquad \textbf{(7)}$$

This correction for the effects of nationwide politics significantly improves the fit to our data. In our graphical presentation of the relationship between M and $N'_s$ further on, we present equations with both the exponents .5 and k as defined in equation 7.

### Step 3. Relation Between the Actual Number of Seat-winning Parties and the Seat Share of the Largest Party

The final independent link in the chain leading to an expression that links $N'_s$ and M is to estimate the relationship between the number of actual seat-winning parties and the seat share of the largest party, $s'_1$. The average fractional seat share of the seat-winning parties must be 1/p′. The fractional share going to the party with the most seats in the given district, $s'_1$, must be at least this average. The upper limit on $s'_1$ is 1, when that party wins all the seats. In the absence of other knowledge, the expected average value is again the geometric mean:

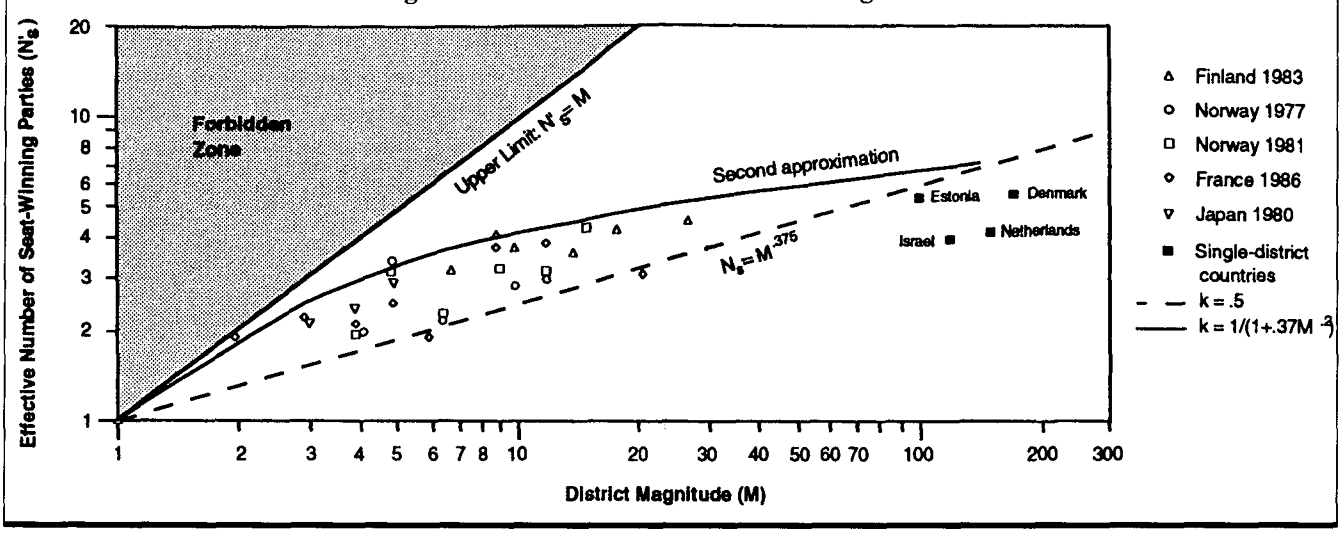
$$s'_1 = 1/p'^{.5}. \qquad \textbf{(8)}$$

This estimate is completely independent of the relationship between p′ and M. Indeed, even if p′ and M were not correlated, equation 8 would still be independently derivable. It should be noted that the largest party in the given district need not be the largest nationwide.

A graphical test of equation 8 showed that the estimate captured the general trend but that data points tended to fall below the curve. Even so, any agreement is quite gratifying, given the simplicity of the assumption that led to the estimation. As before, this simple first-approximation model (equation 8) can be improved upon. The upper limit on the seat share of the largest party is actually *less* than one (rather than one, as is assumed in the first approximation).[5] The test of whether the agreement of either the original model or the second approximation is satisfactory or not will come when we bring together all the building blocks and test our resulting estimation of $N'_s$ based on M against actual data.

**FIGURE 1**

**Effective Number of Seat-winning Parties in District Versus District Magnitude**



## Combining Steps: The Relation between District Magnitude and the Effective Number of Seat-winning Parties in a District

It is now time to combine all the previous steps and express $N_s'$ as a function of district magnitude. The first-approximation model[6] leads to

$$N_s' = M^{.375}. \qquad (9)$$

Figure 1 shows the curve that results from this equation along with data. The actual data points tend to be above this line.

Let us now introduce our corrections. The curve labeled "Second approximation" takes account of the corrections discussed above for both p and $s_1$.[7] Most actual data points fall between the two curves, suggesting overcorrection or an effect of *two-party competition* that will be discussed later.

*Discussion of Results: The Overall Pattern.* Two objections may be raised at this point: (1) such a probability-based model completely ignores the political processes that lead, through elections, to the winning of seats; and (2) it does not even fit the data very well. The two objections actually neutralize each other. If all the data points fell perfectly on the model-predicted line, then, indeed, the political process would be out of the picture. Only institutions would matter—indeed, only one institution, the district magnitude. The very scatter of the points in the actual picture indicates the workings of the various political processes. The range of possible outcomes is nonetheless restricted. No data could be in the "forbidden zone" in which $N_s' > M$, but all the rest of the space of the figure is fair game. Yet all the data are found in a narrow zone. This should not surprise us. Political processes do not take place in a vacuum. They and their outcomes face various restrictions, including logical constraints, of which statistical probabilities are an aspect. The political institutions (particularly the district magnitude) are a critical element providing these constraints on possible political outcomes. Our task here is to define the average pattern and find out the extent of the actual playroom left for political processes.

The achievements of the simple models shown in Figure 1 should not be underestimated. All the data points could conceivably have fallen much above or much below the predicted line. That this is not the case represents success, for a first approximation.

*Poor Fit of the Single-District Countries.* The lower values of $N_s'$ for single-district countries (those in which all seats are allocated in one nationwide district) might imply that for such systems, there is some ceiling for a given M above which the effective number of parties does not rise. A reason for such a reduction below the theoretical prediction could be that most of a country's ambitious politicians (those who desire to sit in cabinets or at least have policy influence) prefer to join one of the larger parties, rather than a very small party. They do so with a greater likelihood than would be predicted on the basis of opportunities for small parties provided by high M. The larger parties' seat shares are thus augmented, so that $N_s'$ is lessened. Thus, although tiny parties based on narrow interests can and do form and win seats, the effective number of parties (high though it may be relative to other countries) is actually *lower* in countries such as Israel and the Netherlands than what M would theoretically allow for. When we get to an estimation of the nationwide $N_s$ including countries with multiple districts, the fit for the single-district countries improves considerably. We now turn to the nationwide scene.

## THE NATIONWIDE NUMBER OF SEAT-WINNING PARTIES

Our process of building a model for the nationwide effective number of seat-winning parties ($N_s$) closely parallels the process used at the district level. Indeed, for one of the building blocks, it matters not at all whether the quantity being measured is district- or national-level. For the relation between the seat share of the largest party ($s_1$) and the number of actual seat-winning parties, equation 8 holds. The other two steps, however, require some fine-tuning at the national level.

### Relation of the Nationwide Effective Number of Seat-winning Parties to the Seat Share of the Largest Party

The basic reasoning for the relationship at the national level between the effective number of parties and the seat share of the largest parties is exactly the same as for a single district for which we derived equation 3: $N_s = s_1^{-1.5}$. However, at the nationwide level another consideration enters, namely, special competition between the two largest parties. The nationwide vote shares of the two largest parties have lately been extremely close in a number of countries (West Germany, New Zealand, Israel, and Switzerland, in their latest election according to Mackie and Rose 1991); and seat shares have also been fairly close. The existence of one large party strongly encourages the opposition to unite or else remain almost permanently in limbo. This can be seen even in Israel despite electoral rules that almost invite the creation of further small parties. Because of such pressures, the second party's vote share has lately been more than one-half of the largest party's in the wide majority of stable democracies, the only exceptions being Denmark, India, Japan, and Sweden. When we consider the effects of two-party competition,[8] we get the following equation for $s_1 < .5$:

$$N_s = .85 s_1^{-1.5} \quad [s_1 < .5]. \tag{10}$$

This correction is not expected to apply to individual districts. The two major parties may compete neck-and-neck for the nationwide total of seats, but why would they throw scarce resources into hopeless individual districts just to try to achieve district-level parity of seats? However, Figure 1 suggests that parties may do exactly that; most data points in Figure 1 would fall between the analogues of the equations developed here to correct for two-party competition at the national level.

### Relation Between Magnitude and the Nationwide Actual Number of Parties

We cannot consider M at the national level in the same way as we do at the district level simply because M, *district* magnitude, is by definition a district-level phenomenon. Except for those systems in which all seats are allocated in one district, the national level forces us to come to grips with the aggregation of several districts. Our reasoning in developing nationwide expressions assumes districts with equal magnitude with all seats allocated in these districts (i.e., no seats allocated in a regional or nationwide tier). Almost no country in the world (with $M > 1$) fits this description; but let us solve the simple case first, then see whether the resulting expressions will work for the more complex systems.

The actual nationwide p is likely to be higher than district-level $p' = M^k$ (equation 5), as some parties win seats in some districts but not in others. If the entire country were made a single electoral district of magnitude S, the number of seat-winning parties would be $S^k$. Thus, $M^k < p < S^k$; and we can again expect the actual values to be around the geometrical average of the possible extreme values:[9] $p = (SM)^{.25}$. At the first glance, this equation suggests that district magnitude and the total size of the assembly are equally important in determining the nationwide number of seat-winning parties; but this is not quite so. The range for S is from about 13 to 650, a ratio of 1:50, while M ranges from 1 to 150, a ratio of 1:150. Hence, M influences the number of parties appreciably more—so much more, in fact, that the effect of S has apparently not been noticed previously.

### Combining Steps: Estimating the Nationwide Effective Number of Parties Based on Magnitude

We can now combine our three steps at the national level just as we did at the district level and arrive at an expression for the nationwide effective number of parties. The resulting expression is[10]

$$N_s = .85(SM)^{3/16}. \tag{11}$$

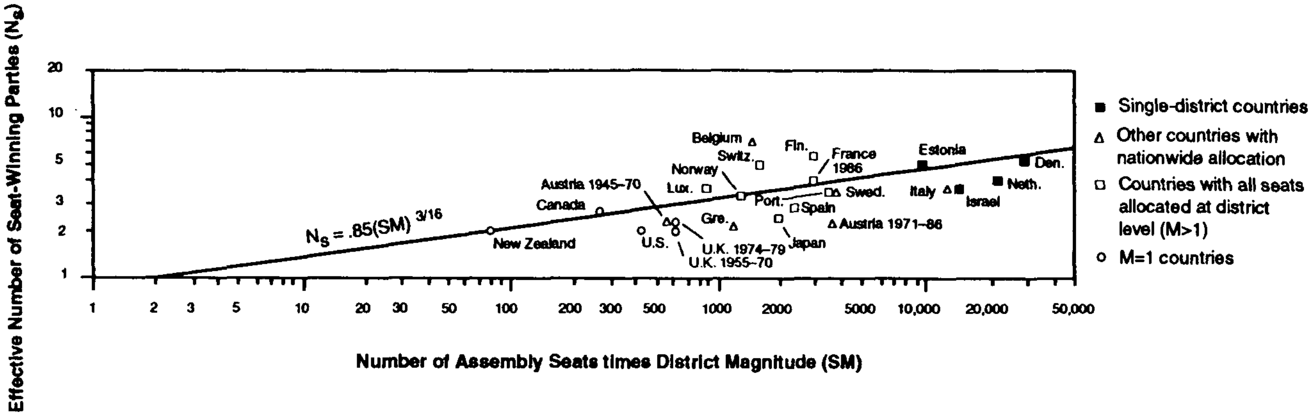
Figure 2 shows basic agreement of this estimation with data. This applies to countries with large assemblies and $M = 1$, as well as to those with smaller assemblies but multiseat districts.

Four different types of countries are distinguished in this graph: (1) nationwide $M > 1$ (i.e., a single district), with a threshold of no more than 2%; (2) $M > 1$, with appreciable supradistrict seat allocation and/or high thresholds; (3) $M > 1$, with seat allocation purely within districts; and (4) $M = 1$ plurality.[11] For the second category, the "effective magnitude" differs considerably from the average district magnitude, which we use for the other cases in which district magnitude varies within the system (see Taagepera and Shugart 1989, 126–41). For the cases with supradistrict allocation and/or thresholds, the agreement of data with estimates based on our simplifying assumptions can be expected to be reduced. However, this does not seem to be the case.

It should be kept in mind that within the same electoral system, the individual outcomes can vary widely. In Finland, for example, the electoral rules have remained the same ever since 1907 (including S and M); and on the average, the theory predicts

**FIGURE 2**

**Nationwide Effective Number of Seat-winning Parties Versus Product of the Number of Assembly Seats and District Magnitude**



$s_1$ = .37 and $N_s$ = 3.7. The actual $s_1$ in Finland has ranged from .25 in 1958 to .51 in 1917, while $N_s$ has ranged from 3.0 in 1917 to 5.5 in 1970. Over the time period taken into account here (1966–87), $N_s$ in Finland has been very close to that predicted by equation 11. Approaches to political science such as that used here imply that over time, party leaders and voters should learn how to play the game as it is established by a country's institutions (Reed 1990). The result of such learning is an adjustment of outcomes, such as $N_s$, to institutions, such as SM. Figure 2 tells us that two institutions—district magnitude and assembly size—rather remarkably account for the effective number of parties in a country. Only a few countries appear as truly deviant. One is Austria after 1971 (although not before then, as we shall discuss); and another is Belgium. Both of those cases deviate by more than 40%. In the case of Belgium, the number of parties has not always been so great; and it could readjust downward once again. Alternatively, parties could choose to reform the electoral system by increasing magnitude, the assembly size, or both, as often happens when the number of parties rises for some time (Shugart 1992).

We have yet to express the effective number of parties nationwide as a function of magnitude alone so as to compare it to the empirical fit in equation 2. Apart from the constraint $S > M$, there is no logical or empirical connection between M and S. As observed earlier, the median S is around 150. Using this approximation to eliminate S from equation 11 leads to equation 12, which is graphed in Figure 3:

$$N_s = 2.15\, M^{3/16} \quad [M < 150]. \qquad (12)$$

For $M = 1$ countries, this equation predicts $N_s = 2.15$, while equation 2 yields 2.3. Throughout the range $1 < M < 40$, the theoretical equation yields values within ±.15 of the empirical equation 2. For larger magnitudes, the theoretical equation yields larger values: for $M = 100$, $N_s = 5.1$, as compared to 4.6 given by equation 2. Both equations express the general trend to an equal degree, and equation 12 can be said to give theoretical explanation and validation to the empirical equation 2, while modifying it to some degree.

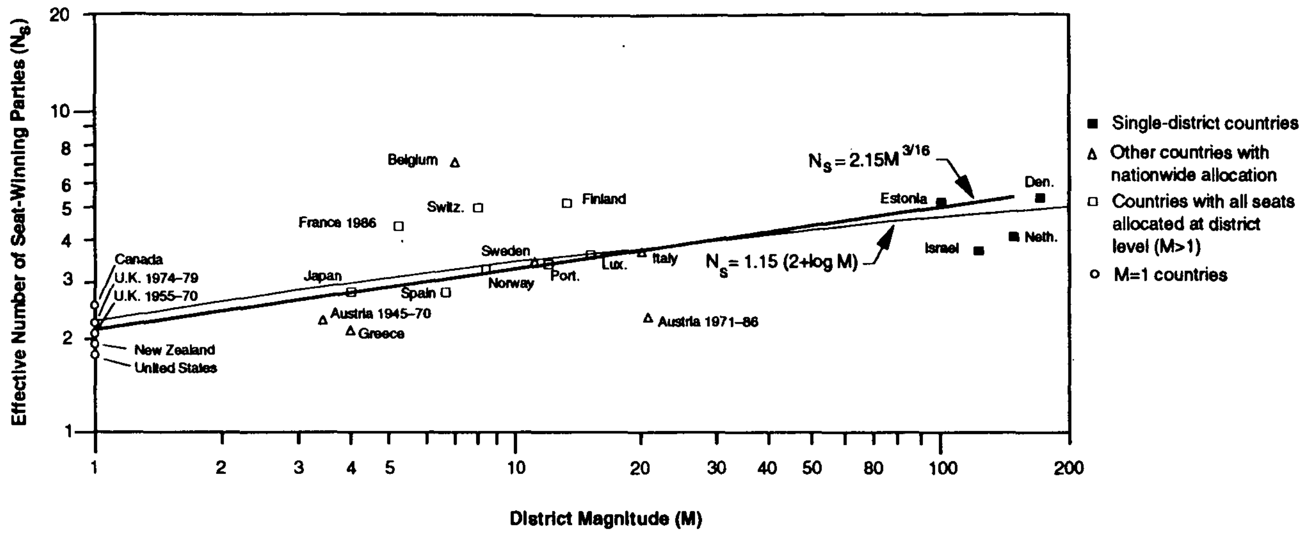
However, there is no need to neglect the role of S. In most cases, a country's assembly size is set first—usually within a narrow range approximated by the cube root of population (Taagepera and Shugart 1989). Once the size of the assembly is set, the magnitude usually follows as a result of how many administrative subdivisions there are—with the exceptions of countries using $M = 1$ or an electoral formula that almost requires small magnitudes.[12] When politicians desire to represent more parties proportionally, they must either increase S (automatically increasing M) or else decide to allocate some seats in a nationwide compensatory district. Thus, the choices of S and M are not entirely independent of one another; but neither does the choice of one value for one variable absolutely restrict the choice on the other variable (unless constrained by, e.g., requiring that all seats be allocated within districts *and* that each preexisting territorial subdivision must form its own district). Compared to equations 2 or 12, equation 11, accounting for S, improves the correlation and is preferable from a theory viewpoint.[13]

## DUVERGER'S MECHANICAL EFFECT: THE QUANTITATIVE MODEL

The Duverger mechanical effect connects the number of seat-winning parties to the district magnitude. For a long time, the theory of this effect has remained on a qualitative level. We have now presented and tested the individual building blocks—or, rather, links in a quantitative concatenation ranging all the way from district magnitude M to the effective number of seat-winning parties on the district and the

FIGURE 3

Nationwide Effective Number of Seat-winning Parties Versus District Magnitude



nationwide levels. Some of these links are firmer than others. We can join them, so as to express everything in terms of M only. While doing so, every link introduces its share of error. In the case of $N'_s$ (district level), the concatenation of simple elements involves three links. For $N_s$ (national level), more links are involved, including the very coarse approximation $S = 150$ if one wants to eliminate S.

We would expect the agreement to become increasingly worse, as we add steps. If any similarity between the final model and the data is found, then the model must be considered quite robust, indeed. This is true, in particular, for the nationwide number of parties, where the average district magnitude is used, although a few very large districts may affect the outcome and supradistrict seat allocation occurs in many countries. In this perspective, ending up with results that agree at all with the general pattern of $N_s$ versus SM must be considered a success.

For the district-level number of parties, Figure 1 suggests that the district magnitude determines the range of outcomes within a factor of two for most individual elections and within a much narrower zone for averages of many elections in the same country. People are certainly free to vote into office many more or many fewer parties in a given district, but it very rarely happens. The implication is, then, that the institutions reviewed here (mainly M, but also S) provide predictable constraints on outcomes. Politicians and voters adjust their behavior in a way that ensures that outcomes accord well with our quantitative predictions.

Where do we find significant deviations from predicted values? We have discussed the cases in which all seats are allocated in one district. These cases deviated on $N'_s$, although not on p′. We suggested also that national politics impinges on individual districts in those countries with multiple districts, leading us to the correction represented by equation 5, taking account of the size of the national assembly. What we find is that national politics impinges upon an individual district even when it is the only district. In work not shown here, we found that for these countries, the actual number of parties (p′) is about what equation 4 predicts; but the effective number ($N'_s$) is less than what equation 9 predicts. This suggests a novel conclusion, namely, that the incentives to control executive office in these parliamentary systems with nationwide allocation leads to *less* extreme fragmentation than the institutional design alone would imply. That a national-level factor is important for these countries, as well as for the rest of the sample, is suggested by the greater fit for these cases to the models derived for nationwide results (equations 11 and 12), which introduce assembly size.[14]

Two cases generally regarded as deviant in the literature on Duverger's law (as reviewed by Riker 1982) deserve attention here. They are Austria and Canada, which will be discussed alongside other contrasting cases. Austria is often thought to be exceptional, because it uses proportional representation yet has only two important parties. The sociological dominance of the two big parties' organizations has been invoked as an explanation (Riker 1982). If we look to the period since 1971, indeed, Austria looks deviant. However, before 1971, Austria's electoral system was based on small districts with restricted access to allocation of nationwide compensatory seats (Kitzinger 1959), such that the effective magnitude was about 3.5 (Taagepera and Shugart 1989, 138). With such a low magnitude, a low number of parties can be expected. The effective magnitude was increased to about 20 in 1971. If

Austria's $N_s$ remains low for a long period of time, then we would indeed need an extrainstitutional explanation. However, recent elections have seen a rise in $N_s$, which may presage a gradual adjustment of the party system to the new electoral rules.

In Canada, the number of parties is often claimed to be "too high" for Duverger's law to be valid. Riker (1982) suggests federalism as the explanation: parties that are based in provinces also run nationally; thus, $N_s$ is appreciably greater than 2.0 despite $M = 1$. However, Canada's $N_s$ is almost precisely what it is predicted to be when S is taken into account. On the other hand, the United Kingdom, almost always the standard example confirming Duverger's law, is actually below predictions (although not enough to categorize it as deviant). Thus, rather than invoking federalism to explain a relatively high number of parties in Canada, perhaps we should invoke *lack of federalism* to explain the relatively low number of parties in the United Kingdom,[15] much as presidentialism can explain why the United States is also low.[16] Federalism need not be invoked for Canada unless $N_s$ rises and stabilizes without a prior increase in S.

Besides the case of Austria since 1971 (already discussed), Belgium is the only case that deviates substantially from predictions even when the assembly size is taken into account. Figure 3 shows Finland and Switzerland also far off that theoretical curve, which does not account for a country's particular assembly size.[17] These cases have "too many" parties for their electoral rules. Yet no one ever calls these countries deviant: they use proportional representation, so they are *supposed* to have a lot of parties! Since none of these countries has always had such high values of $N_s$, it is possible that either their values of $N_s$ will at some time decline once again or else the institutions will be reformed. If either event resulted, it would indicate a long-term adjustment of behavior and institutions to one another. The models here do not, however, allow us to predict when such adjustment would occur or, indeed, even if it will occur. This discussion has suggested that a promising next step would be longitudinal analysis of individual countries, to look more directly at the process of adaptation (see Reed 1990; Shugart 1992).

## CONCLUSIONS

What have we achieved? We have asked simple questions like, How many parties might one expect to see in an electoral district? These simple questions have apparently not been asked previously, largely because attention has been fixed on the national level. The national level is more important, but it is not conducive to model building (without looking first into district-level phenomena); hence, analysts were left with primarily empirical expressions. On the district level, simple models can be constructed and then expanded to the nationwide. The simplest is to establish the upper and lower boundaries of a variable and assume that average values in between these boundaries prevail. This approach has been quite successful: the model agrees with the data surprisingly well.

The interaction between empirical work and model building is a two-way street. In the present case, empirical results at time urged a search for a theoretical explanation, while at other times theoretical advances suggested that unsuspected relationships be tested empirically.

There is a broader question about this exercise that is worth pondering. That is the question of what these models really tell us about politics, as opposed to simple numerical descriptions of average political outcomes. These models should not be construed as apolitical, for two reasons. First, at least as far back as Duverger (1951, 1954), the connection between electoral systems (mainly defined by their district magnitude) and the number of parties represented in assemblies was recognized as "mechanical." Since political institutions, including district magnitude, are a machine that converts the actions of politicians and voters into political outcomes, it is hardly surprising that models of the effect should themselves appear "mechanical" in form.

Second, we have not offered any models of how individual politicians calculate costs and benefits, as do some rational-choice theorists in political science. Those are micro concerns and involve forays into what Duverger termed the "psychological" effect. Our models suggest that the aggregation of individual political actors' calculations, made in part according to the institutional context in which winning or losing will be determined, occurs in predictable fashion. Perhaps some day we may have models of how this aggregation itself takes place. The remarkable regularity across countries in their political outcomes (at least in terms of the number of parties) according to the form of their political institutions gives us some hope that we have taken a step in the direction of such more generalizable models of politics.

Nor have we advanced herein a theory of how electoral rules are chosen, although we have suggested that where the existing number of parties is "too high" for the existing magnitude, a "psychologically" induced reduction in the number of parties is not the only possible outcome. Indeed, electoral reform might result, increasing magnitude. Further theoretical and empirical work should address itself to the interaction among the mechanical effect, psychological effect, and choice of electoral rules. We have made a preliminary report on the first part of this link.

## Notes

1. Indeed, N is merely a transformation of Rae's index of fractionalization: $N = 1/(1 - F)$. We prefer N because it is easier to visualize in concrete terms. For example, $N = 2.84$ suggests that there are more than two, but definitely less than

three, major parties, while F = .648 less obviously corresponds to the number of components in the system.

2. N is not the only measure that could be used. See Laakso and Taagepera 1979 and Taagepera and Shugart 1989 for a discussion.

3. Sabermetrics is the quantitative analysis of regularities in aggregate performance in baseball. Although it has (yet) to claim a single university department of its own, it is indeed a "scientific" discipline with sophisticated methods seldom seen by (or of any interest to) most fans or sportwriters. See James 1984, 9–24, and *Baseball Abstract* in general.

4. The geometrical mean is computed by multiplying the values of the upper and lower extremes, then taking the square root of the product. It may be thought of as an arithmetic mean in log–log form, which we have used here because, in order for an assumption of linearity to work on these data, one must first transform the data through decimal logarithms.

5. The reason is that if more than one party wins seats in a district, the largest party obviously can not win 100% of the seats. In a district with M seats, if p' parties win at least one seat each, then the largest party cannot win M seats but only M − p' + 1 at the most. Its maximum share of the seats then is (M − p' + 1)/M, rather than 1. Since M can, in principle, be expressed in terms of p'—by reversing the directions of equations 5 and 8—$s_1'$ remains a function of p' alone, as it was in equation 8. However, the algebraic form of the relationship is quite complex. Rather than finding it, it is more fruitful to determine the second-approximation curve by calculating various points on it. For instance, for M = 9, equations 5 and 8 yield p' = 4.04 and maximum $s_1'$ is (9 − 4.04 + 1)/9 = .66 (rather than 1). The geometric mean of the extremes becomes $s_1' = (.66/4.04)^{.5} = .40$ (rather than .50, as given by equation 8). Calculations of this sort yield a "second approximation" that visibly improved the agreement with data.

6. This first approximation is derived from the aforementioned building blocks, as follows. Our goal is to estimate $N_s$ as a function of M. The one variable that we were able to estimate deductively as a function of M was p, so we start there: $p = M^{.5}$. We also estimated that $s_1 = 1/(p^{.5})$; therefore, $s_1 = 1/[(M^{.5})^{.5}] = 1/(M^{.25}) = M^{-.25}$. Now we return to $N_s = s_1^{-1.5}$ and plug in our estimate of $s_1$ in terms of M: $N_s = (M^{-.25})^{-1.5} = M^{.375}$.

7. Equation 9 may be respecified as $N_s = M^{.75k}$, where k is defined as .5 in the first approximation (derived from equation 4) or according to equation 7 in the second approximation.

8. The derivation of this estimation is as follows. In the case of perfect two-party competition ($s_2 = s_1$), the upper limit on $N_s$ is no longer $1/s_1^2$ but $1/2s_1^2$, while the lower limit remains $1/s_1$. The geometric mean of the extremes becomes

$$N_s = 2^{-.5}\, s_1^{-1.5} = .71 s_1^{-1.5} \quad [s_1 < .5]. \qquad \textbf{(13)}$$

A graphical analysis showed that, for $s_1 < .5$, the actual data points fall largely in the zone delineated by equation 13 and equation 3, that is, in the zone between full two-party competition and no two-party competition. The average pattern for $s_1 < .5$ is the mean of the two pure cases: $N_s = .85 s_1^{-1.5}$. For $s_1 > .5$, the second-largest party must be smaller than the largest one, and equation 13 is no longer valid.

9. We use k = .5 instead of the more complex form given by equation 7. The latter expresses the effect of nationwide politics on the district, and using it here would mean correcting the nationwide politics for itself.

10. The algebraic derivation proceeds as follows. We have estimated that $s_1 = (1/p^{.5})$ and that $p = (SM)^{.25}$. Therefore, $s_1 = 1/[(SM)^{.25}]^{.5} = (SM)^{-.125}$. Plugging this into equation 10, we get $N_s = .85[(SM)^{-.125}]^{-1.5} = .85(SM)^{.1875} = .85(SM)^{3/16}$.

11. The countries and time periods used, by category identified in the text, are (1) Denmark, Estonia (1923–32), Israel, and the Netherlands; (2) Austria, Belgium, Greece, Italy, and Sweden; (3) Finland, France (1986), Japan, Luxembourg, Norway, Portugal, Spain, and Switzerland; (4) Canada, New Zealand, the United Kingdom (1955–70, 1974–79), and the United States. Median values are used from the last seven elections listed in Mackie and Rose 1991 except as indicated or where a change of electoral system had occurred during the period.

12. The single transferable vote (as in Ireland) and single nontransferable vote (as in Japan) are rarely used with districts with magnitudes much greater than six.

13. The previous empirical expression (equation 2) did not include S because its effect could not be observed easily, overshadowed as it was by the effect of M.

14. Since all the nationwide cases use S of around 150, the fit to the model that introduces this average value (equation 11) is no worse than that of the model that uses each country's specific value of S (equation 12).

15. If the United Kingdom ever implements a home rule parliament for Scotland, we will have a test. With a local parliament as a "prize," perhaps the Scottish National party would perform better. If this were carried over into national elections, $N_s$ would increase somewhat.

16. Presidential systems can have a considerable and varied effect on the number of parties in the assembly (Epstein 1967; Shugart 1988); and the present simple theory does not take this effect into account. See Shugart and Carey 1992, 230–37, 293–300 for related models that do account for the "presidential difference."

17. France in 1986 looks deviant in Figure 3; but when its large S is accounted for (Figure 2), the model accounts almost perfectly for the effective number of parties in that system.

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# NOTES FROM THE MANAGING EDITOR

**Forthcoming in September, 1993**

The following articles, research notes and controversies have been tentatively scheduled for publication in the September 1993 issue:

Gary W. Cox and Frances Rosenbluth. "Electoral Fortunes of Legislative Factions in Japan."

Raymond M. Duch. "Tolerating Economic Reform: Popular Support for Transition to a Free Market in the Former Soviet Union."

Walter Enders and Todd Sandler. "The Effectiveness of Anti-Terrorism Policies: A VAR-Intervention Analysis."

Paul Huth, Christopher Gelpi, and D. Scott Bennett. "The Escalation of Great Power Militarized Disputes: Testing Rational Deterrence Theory and Structural Realism."

John E. Jackson and Elisabeth Gerber. "Endogenous Preferences and the Study of Institutions."

Bryan D. Jones, Frank R. Baumgartner, and Jeffery C. Talbert. "The Destruction of Issue Monopolies in Congress."

George E. Marcus and Michael B. MacKuen. "Anxiety, Enthusiasm, and the Vote: On the Emotional Underpinnings of Learning and Involvement During Presidential Campaigns."

Zeev Moaz and Bruce Russett. "Normative and Structural Causes of Democratic Peace, 1946–1986."

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Jon A. Krosnick and Laura A. Brannon. "The Impact of the Gulf War on the Ingredients of Presidential Evaluations: Multidimensional Effects of Political Involvement." A Research Note.

Kevin T. McGuire and Greg A. Caldiera. "Lawyers, Organized Interests, and the Law of Obscenity: Agenda Setting in the Supreme Court." A Research Note.

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## ERRATUM

"Politics, Institutions, and Welfare Spending in Industrialized Democracies, 1960–82" by Alexander M. Hicks and Duane H. Swank, September 1992, pp. 658–674. On page 659, column 2, the sentence beginning the last paragraph should read "Scholars within the political democratic tradition have downplayed effects of partisanship and stressed the social welfare impacts of such nonpartisan facets of electoral systems as electoral participation and competition (Dye 1979; Pampel and Williamson 1989)."

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# POLITICAL THEORY

**Power and Empowerment: A Radical Theory of Participatory Democracy.** By Peter Bachrach and Aryeh Botwinick. Philadelphia: Temple University Press, 1992. 211p. $44.95 cloth, $16.95 paper.

In this excellent book, Bachrach and Botwinick call for the pursuit of a participatory democracy through the pursuit of workplace democracy. The politics of workplace participation must move beyond "protective rights" to assert "participatory rights," involving decisionmaking by workers in the traditional affairs of capital. In so doing, class can become a unifying category for overcoming worker passivity and for building a participatory democracy.

In making their argument, Bachrach and Botwinick make at least two important contributions to the literature. First, as Bachrach has done in earlier works, the authors ground their prescriptive argument in a refreshing discussion and analysis of the nature of participation. They distinguish the "participationists" view of democracy from what they call the liberal representative view (earlier called the pluralist view.) Reviewing various criticisms of the ideals of participatory democracy that have been made over the years, they argue that "the underlying premise of participatory democracy—in contrast to liberal representative democracy—is that participatory democratic politics encompasses self-exploration and self-development by the citizenry." (10–11). In sharp contrast, liberal doctrine conceives of democracy as merely facilitating passively the expression of perceived interests, not in helping citizens discover what their real interests are. From the participatory view, to the extent that people become involved in the political process, the process will be educative and help in turn to forge new levels of involvement and awareness. In this sense, participation is not only the result of political consciousness, it helps to create it.

A second important contribution that the authors make is to link this argument about democratic participation to that of a discussion of class and of class struggle. Bachrach and Botwinick challenge the failure of democratic theorists to deal with questions of class. Even theorists like Dahl, who have taken up arguments for economic democracy, are chastised for ignoring how the theory will be implemented through worker action. In so doing, they argue, democratic theorists have left the question of class to be answered either by the right or by traditional Marxist politics. Growing economic inequalities and the rise of corporate power in the 1980's mean that class can no longer be ignored, and the decline of Cold War politics offers the possibility of redefinition of class struggle in democratic terms.

The political solutions to economic inequality must go beyond New Deal ones, for the welfare-state strategies do not deal with the "central problem that confronts American democracy—the maldistribution of economic and political power.(9)" The solutions will more fruitfully be generated by a new class struggle for worker participation and decision-making. Because of the educative nature of participation itself, class struggle would also help to solve another critical problem of the time—political alienation. The process of struggle for workplace democracy becomes just as important as the goal and would, if bold enough, become the vehicle for a revitalization and democratization of American politics. Rather than being anti-democratic, a class-based movement for participation can in fact help democratic pluralism to flourish: "it should be welcomed and encouraged as a way to revitalize our failing democratic polity, as a way to realign parties along class lines and thus generate expanded citizen participation and public consciousness of issues of national concern.(17)"

The authors support their analytical and prescriptive discussion with two chapters which explore current experiences in workplace democracy in Europe and in the United States. They are critical of much of what they see. In Germany and Sweden worker participation may have strengthened the role of the union, but not necessarily the participation of the workers themselves. In the United States the obstacles include not only corporate power but also union power. Only as unions recognize that the key to effective power in the future is to extend decision-making in the workplace will they be able to adapt to the changing technological conditions that have eroded their traditional bargaining strength.

To build a movement for worker participation, one must confront the paradox that has faced all participationists: those most likely to participate by virtue of their economic position as workers are also most affected by a structural mobilization of bias that impedes their participation. By moving participatory demands from the state to the workplace, the authors hope to bypass some of the class-based bias against participation that has affected traditional interest group politics. But, they fail to discuss the strongest impediment to such worker demands in today's economy—that of the ability of capital to play global hopscotch with the communities and workers who offer the most favorable climate and circumstance. Without state-imposed limits on the global mobility of capital, corporations are likely to evade mass demands for worker participation in all but the most ailing industries. And, if workers gain true participation in only the firms that management has discarded, their participation will still be second-class and ultimately unequal.

The book is stronger in its theoretical analysis and prescription than in its empirical description. Nevertheless, it provides a welcome addition to the participatory democracy literature—a literature whose shelf has become somewhat barren in recent years.

*University of Tennessee* JOHN GAVENTA

**Social Division.** By Alan Carling. New York: Verso, 1992. 442p. $69.95 cloth, $22.95 paper.

For roughly the first third of *Social Division*, Alan Carling, (following John Roemer's (1982) *A General Theory of Exploitation and Class*) argues that rational choice (by which, in this case, he means game theoretical modeling) in conjunction with G. A. Cohen's (functionalist) reconstruction of historical materialism and Robert Brenner's account of class struggle, explanatory strategies that also rely on notions of rational choice explains class divisions. He then goes on to hold that game theoretical modeling is indispensable but insufficient for explaining social divisions based on gender and ethnicity. It is not clear why he concludes that it is indispensable; apparently it is because he is able to contrive models that make sense of some aspects of phenomena pertinent to gender and ethnic divisions. He thinks rational choice insufficient because, in each case, the best model for these social divisions turns out to be "chicken," a game with indeterminate outcomes. Carling therefore concludes that social divisions cannot be explained by rational choice alone and that, at least for gender and ethnicity, supraindividual systems of perception and belief must also be taken on board. In short, economics must be supplemented by sociology.

These conclusions are easily lost, however, in extended arguments about the propriety of one or another game theoretical reconstruction and in lengthy expositions of a considerable body of writings—not just by Roemer, Cohen, Brenner, and other "analytical Marxists," but also by Paul Samuelson, Michael Banton, Susan McRae and, more briefly, a host of other writers. The efforts at modeling are sometimes ingenious, frequently tedious; the expository sections are consistently competent, but readers who are already familiar with the material under discussion are unlikely to learn much—except, of course, what Carling thinks about the positions he recounts. These readers too will be put off by some very sketchy critical reflections of an idiosyncratic nature: for instance, by the assertion that Cohen's and Brenner's views about the origins of capitalism are expressly opposed, and then by his brief reconciliation of these ostensibly competing positions. On the other hand, readers who are unfamiliar with the positions exposited may find Carling's accounts useful and even eye-opening. They could, however, turn directly to the material he discusses.

Carling's account of class divisions is at odds with the standard Marxist story, but it is almost entirely derivative from the reconstructions and elaborations of Marxist positions developed by analytical Marxists in the past decade and a half. His accounts of gender and ethnicity are more original, but the conclusions he draws should surprise no one except perhaps the most ardent believers in rational choice explanations. The treatment of ethnicity, essentially a critique of Michael Banton's (1983) *Racial and Ethnic Competition,* is well-argued and insightful. However, the discussion of gender is more problematic. Carling's aim is evidently to do for gender what Roemer has done for class. But here, above all, he seems to have lost sight of the forest (the explanandum) for the trees (the game theoretic models he advances). Most of the time, what he seems to want to account for is just the division of household labor in two person households where the individuals in question have (for whatever reason) differential earning capacities. Needless to say, this explanandum has only a tenuous connection to the concerns of those who have brought questions about gender into the forefront of contemporary discussions.

Carling remarks at the outset that he once thought rational choice could explain the general phenomenon of social division, but that he has now adopted a more pluralistic view. *Social Division* can therefore be read as an attempt to discern the forms and limits of rational choice explanations. From that perspective, it makes a useful contribution to continuing debates within the social sciences.

*University of Wisconsin, Madison* ANDREW LEVINE

**Willful Liberalism: Voluntarism and Individuality in Political Theory and Practice.** By Richard E. Flathman. Ithaca: Cornell University Press, 1992. 232p. $31.50 cloth, $13.95 paper.

This is an elegant and studied little volume, rather more difficult than it lets on. Flathman wants to argue that liberals are sorely in need of a more robust understanding of the will and individuality than they now possess, that they (or we) should be enthusiastically embracing what might seem to be some tendentious commitments about the partial but inescapable opacity of other selves. He does so by working through a large number of texts and authors—some only contentiously called liberal (Hobbes); others not conceivably liberal (William of Ockham, Augustine, Nietzsche); and still others not obviously interested in anything narrowly political at all (Wittgenstein, William James). The exegeses, sometimes dense and always deft, are aimed at excavating and reclaiming that robust understanding we allegedly lack. So they are not in the first instance intended for, say, Hobbes scholars or James scholars.

Flathman does not quite explain his impatience with reigning liberal theories. I take it his sense is that there is too much sweet reasonableness, too much of a community dedicated to shared principles of justice and public debate. But this vision is not just an implausible attempt to fashion society as a learned graduate seminar. It is also a bit soporific; and one would have to worry that the individuals composing it were bland, colorless, interchangeable. Or so, I conjecture, Flathman thinks.

That may make it sound as if Flathman thinks that individualism (or "individuality") is somehow the opposite of being socially situated (and surely many recent antiliberals write that way). What emerges from Flathman's juggling of these notions (under titles like "Individuality and Plurality, Sociality and Politicality," "Sociality, Individuality, Plurality, and Politics," etc.) is a sharp riposte to any such misbegotten notion. Flathman may want to celebrate partially opaque individuals with eccentrically strong wills; but he realizes full well that only in certain kinds of communities or social orders can such individuals emerge in the first place. Readers who still believe that liberalism is somehow a presociological or antisocial doctrine would be well advised to dwell on this strand of his argument.

There is something mildly paradoxical about the structure of Flathman's position. Think of a theory as a web of beliefs and leave it an open question (an "empirical question," as they say in the trade) how densely tangled together the strands of the web are and whether some parts of the web are relatively independent of others. Hence arises an antinomy such that while theories of individuality must be independent enough that we can imagine lifting them from Ockham and inserting them into liberalism, they must not be so independent that they are just freely spinning gears stuck on the side of the real conceptual machinery (or else inserting them will not matter).

There is, indeed, logical room for that. But at the very least, one would like to hear more about what "appropriating" or "inserting" these commitments into liberalism would mean. Nor, presumably, is a liberal (or any) web of beliefs, built like a Motorola color television, so that we might just snap out the old defective module labeled "views of self and society" and insert the new one. Presumably, that is, inserting these commitments will somehow change the rest of liberalism if only by putting other liberal commitments in a new context. But Flathman does not take up the constructive task of even sketching what our new and improved liberalism will look like in any serious detail.

I do not mean to catalog the failings of Flathman's book, but to suggest how much it opens up for us, how much further work along these lines might be done. In a debate as viciously repetitive as that between liberals and their critics, the act of putting a quirky, talented, provocative sketch on the table is remarkable.

*University of Michigan* DON HERZOG

**Eric Voegelin and the Politics of Spiritual Revolt: The Roots of Modern Ideology.** By Michael Franz. Baton Rouge: Louisiana State University Press, 1992. 169p. $27.50.

Michael Franz has produced a welcome illumination of Eric Voegelin's political philosophy. His book is "an examination of Eric Voegelin's analysis of the spiritual and historical roots of modern ideological politics" (p. ix)—no mean task. Given the scope of this project, his observation that the several readers of earlier drafts "each . . . has objected to at least one aspect of my interpretation" (p. xi) is not surprising. In the space of seven brief chapters, however, Franz achieves the aim of explicating Voegelin's long-term project of analyzing, diagnosing, and prescribing a therapeutic for the modern problem of "pneumopathological disorientation."

This disorder was understood by Voegelin to be a spiritual pathology in which one's dissatisfaction with the human condition as it is given to us leads one to speculate on the possibility of a radical change. Franz enumerates four kinds of pathological response to this experience of dissatisfaction with, or anxiety over, the world's contingent nature (pp. 7–11). Each kind is animated by a different sort of hope or program for radical change in the basic structures of the world and its transcendent ground. Voegelin's analysis was generally not an explication of specific ideological doctrines but an examination of the experiences the human psyche has of the world and its transcendent ground and how these experiences and responses to them are revealed symbolically in human history. The various responses of the psyche can be translated into political symbols and deeds. A fundamental premise of Voegelin's enterprise, therefore, was that some responses are better than others: psyches can be either ordered or disordered. The relative states of health or pathology in the psyches of the dominant participants in a régime are reflected in the general character of the régime. Franz outlines well the weaknesses of this argument first articulated by Plato but shows how it might be defended, given its centrality to Voegelin's analysis of political phenomena and, especially, ideology. A disordered psyche may, in one way or another, attempt to contract the world and its transcendent ground, seeking hypothetically or actually to gain control over them. The resulting political ideas and actions range from the laughable to the horrifying.

To synthesize a long, productive lifetime of scholarly work in a book of this modest length will have obvious limitations. Franz cannot hope to reflect the richness of Voegelin's account, yet he marshals the salient arguments in such a manner that he cannot be accused of superficiality. He is sensitive to many of the shortcomings in Voegelin's project, discussing, defending, or acknowledging them in numerous places (e.g., pp. 4–20, 102–6, 111–3, 123–35). His efforts are a refreshing exception to Whitehead's dictum that the greatest enemies of discovery are the disciples of discoverers.

As in much of the secondary literature on Voegelin, there is almost no discussion of his early writings. They are essentially relegated to notes. To regard them as insufficient because they are merely an "analysis of doctrine," rather than of "engendering consciousness" (p. 116, n. 17), however, seems somewhat misled. Voegelin listed *Die Rassenidee*, for example, among his "best efforts"; Hannah Arendt praised the two race books highly in her *Origins of Totalitarianism* (1973); and Franz himself points to a lack of further such detailed studies as an unfortunate gap in Voegelin's corpus (p. 127). Moreover, several of these early works already begin to address problems of consciousness and engendering experiences and their symbolic content in ways that are not merely "analysis of doctrine." Most importantly, this marginalizing treatment obscures the ever-present philosophical obligation not only to understand the basic "engendering consciousness" of the ideologist but also to know the specific distortions that such a consciousness produces. The social and political analyses of several of Voegelin's pneumopathic figures provide us with rich insights alongside their destructive visions (Nietzsche, Hegel, and Heidegger, to mention three). To give them their due (which may be a lot), we require not only a general account of deformed and healthy consciousness but also an account of what specific thinkers are up to and of how a well-ordered soul might respond.

Finally, the book is both accessible and inaccessible at the same time. Voegelin developed a very precise but somewhat obscure conceptual vocabulary during his life-long investigation of political phenomena. When Franz introduces concepts such as *metaxy* or Second Reality (borrowed from Robert Musil) without a clear explanation of their meanings, he likely loses the uninitiated. Since he hopes to make "Voegelin's work on ideology and gnosticism more accessible and useful to those new to his writings" (p. x)—an intention that is enhanced by an extensive note apparatus but somewhat encumbered by an inadequate index—such slips are unfortunate. In this one respect, Franz's generally clear and straightforward presentation is somewhat unfriendly to the reader not closely familiar with Voegelin's work.

This book, despite these minor faults, will earn a prominent place in the growing body of secondary literature on Voegelin's thought and work. It is a valuable, comprehensive account of his political philosophy.

*University of Kansas* THOMAS HEILKE

**Philosophical Anarchism and Civil Disobedience.** By Chaim Gans. New York: Cambridge University Press, 1992. 174p. $44.95.

The mainstream position on questions of political obligation is that all citizens have obligations to obey the law but that these are of limited force. To employ terminology introduced by W. D. Ross, theorists generally argue that political obligations are prima facie. They bind, other things being equal, but can be overridden by strong countervailing considerations, in which case citizens need not obey and may have recourse to alternatives ranging from civil disobedience to armed resistance. In recent years, the mainstream position has come under attack, as common justifications for political obligations have been scrutinized and found wanting. Increasingly popular is a position Chaim Gans calls "critical anarchism"—that there are no political obligations because the arguments that support them are indefensible.

In *Philosophical Anarchism and Civil Disobedience,* Gans attempts to defend an approximation of the mainstream position from recent criticisms. His central effort is two-sided. Along with critical anarchism, the traditional view has been attacked by "autonomy-based" anarchism, the view that an open-ended requirement to obey the law (whatever this turns out to be) is incompatible with citizens' moral autonomy. Gans criticizes the conceptual basis of this form of anarchism in chapter 1, arguing that its proponents exaggerate the surrender of autonomy political obligation entails. Chapter 2 addresses critical anarchism. Four arguments in favor of political obligations are, in part, salvaged from recent criticisms: fairness, adverse consequences of disobedience, duty to support just institutions, and what Gans calls "communal obligation." These constitute a "complex whole," able to ground obligations, "albeit a somewhat weaker duty than is commonly believed" (p. 43). In chapter 3, this conclusion is bolstered by additional considerations peculiar to just democracies. In the final chapter, Gans turns to circumstances under which political obligations do not hold. His overall position is that they can be overridden for good moral reasons, though they take precedence over amoral reasons, notably self-interest.

There is no doubt that *Philosophical Anarchism and Civil Disobedience* is a worthwhile book. It is thoughtful, forcefully argued, and in parts original. The questions it addresses are of great concern to scholars and citizens alike. Because it defends a familiar view, much of the book's argumentation is also familiar; but it is conducted at a high level and studded with interesting insights. Many particular discussions are elegant, concise, and instructive. Some examples are the analyses of different sorts of authority (pp. 33–41) and reasons why obligations to unjust governments are void ab initio (pp. 96–108). Another example is Gans's attempt in chapter 4 to distinguish the points of view of citizens and authorities in regard to the limits of obedience.

The book's main flaw is unfortunate brevity. Many problems, once raised, are rushed through, with loose ends and possible objections skirted or ignored. For instance, in chapter 1, Gans rejects the view that political obligations are prima facie, arguing, instead, that they are what he calls "practical musts of limited absoluteness" (pp. 19–21). However, his brief objections to the prima facie view could be easily countered. His view depends on the important distinction between amoral reasons, which political obligations outweigh, and moral reasons, which they do not. But this receives a single paragraph that raises more questions than it answers. More serious, Gans's treatment of the grounds of political obligation is closer to a sketch than a full-blown analysis of the issues. Six possible bases for political obligations are covered in 52 pages. The four that survive are not carefully formulated or defended from possible objections. For instance, little or nothing is said about the nature of the natural duty to support just institutions, why it binds, or how it is able to require such onerous burdens of citizenship as paying taxes and providing military service. The argument from consequences is defended through the interesting claim that it does not bind in regard to all laws. But among problems Gans fails to address here is that the laws it appears not to support include those requiring the central burdens of citizenship just noted. Gans claims that political obligations rest on a complex of overlapping moral principles; but since the individual principles are not carefully formulated, exactly how do they interact?

Important aspects of the chapter are out of date. For instance, Gans's main criticism of gratitude as a basis for political obligation has been countered by A. D. M. Walker's "Political Obligation and the Argument from Gratitude" (*Philosophy and Public Affairs* 17[1988]). All these problems with the grounds of political obligations also undermine Gans's discussion of civil disobedience, since this depends on "the inner logic of the foundations of the duty to obey" (p. 120).

In defending the traditional position on questions of political obligation, Gans helps recapture important philosophical territory. He resolutely tackles a tangle of controversial issues, clearing up some, shedding new light on others. If not everything he says is adequately defended, one can hope that his future work will elaborate his more interesting themes.

*University of Virginia* GEORGE KLOSKO

**A Culture of Rights: The Bill of Rights in Philosophy, Politics, and Law, 1791 and 1991.** Edited by Michael J. Lacey and Knud Haakonssen. New York: Cambridge University Press, 1991. 474p. $49.95.

**The Constitution of Rights: Human Dignity and American Values.** Edited by Michael J. Meyer and W. A. Parent. Ithaca: Cornell University Press, 1992. 248p. $32.50 cloth, $12.95 paper.

The bicentennial of the Bill of Rights has inspired a number of scholarly reassessments of the nature of rights and their role in the American constitutional order past and present. Among them are the wide-ranging essays of philosophers, political theorists, historians and lawyers collected in these two fine volumes. The historical essays therein (e.g., by Rakove and Hutson in Lacey, ed.) show that rights talk was no less prevalent—nor less contested—at the founding than at present. The philosophical ones, on the other hand, suggest that appeals to rights have been useful in defending particular freedoms despite continuing disagreement as to the proper ways to justify the existence, content or scope of given rights much less a theory of rights in general. Taken together, these books can add much to the theoretical breadth and depth of what is too often the overly parochial field of American political thought.

The unifying theme of *The Constitution of Rights* derives from a remark of Justice William Brennan, Jr. who has described the Constitution as a "sublime oration on the dignity of man." (p. 47) The book is also no doubt inspired by the revival of Kantian political theory in the works of Rawls, Dworkin, Gewirth and others wherein a conception of dignity (if somewhat idiosyncratically understood) has a central, even foundational, place. Its contributors were asked to relate the notion of dignity to general theories of rights or to particular constitutional rights such as those to free speech and equal protection or that against self-incrimination.

One difficulty in adopting this focus in a work structured around the American Bill of Rights is that references to dignity are sparse in American founding documents. Thus, one task is to read this moral idea into the Constitution as a principle that helps make sense of the particular rights claims that are there. There is nothing wrong in doing this, originalist objections notwithstanding, though there is some reason to question its utility. Jefferson, at least, could assert as a self-evident truth that men are born with certain inalienable rights without claiming that those rights needed further justification in terms of dignity.

There may be good reason not to follow Jefferson's lead on this. It is a common view in contemporary moral philosophy that rights require justification in terms of some other moral criterion. Nonetheless, it is an irony of this volume that the essays most closely concerned with constitutional practice and interpretation tend to take dignity much less seriously as an organizing principle than do those with a more distanced philosophical approach. Thus, Frederick Shauer's essay on free speech is openly skeptical on the possibilities of a defense of speech rights grounded in dignity. And, other authors hitch dignity to some other concept which is doing the real justificatory work in their arguments. This characterization applies to Martha Minnow's discussion of equality (though one is hardpressed to find any thread of principle uniting her interpretive preferences) and to David Richards' more effective piece on the centrality of reasoned justifications of the exercise of political power in liberal constitutional regimes.

The debate between Alan Gewirth and A. I. Melden on the relation between dignity and rights is instructive whatever its relevance to American constitutional practice. Gewirth's essay applies arguments he makes elsewhere for the existence of human rights to the question of dignity. At its most basic, this argument claims that all agents seek objects that they (must) consider worth pursuing. The agent rightly concludes that his ends are worth attaining "because *he* is worth sustaining and fulfilling." (p. 22) Therefore the agent considers himself as worthy, or as possessing an inherent dignity. Through universalization, he ascribes this quality to all rational agents and it grounds rights which are needed to make effective nonharmful rational action.

Gewirth's account trades upon two senses of the notion of worth. How it is that an amoral notion (e.g., the glutton believes another eclair is worth having) is converted to a moral one (e.g., therefore the glutton is morally worthy) is not adequately spelled out. A suspicion that it cannot be worked out leads Melden, and other virtue-based moralists, away from "Kantian" dignity toward a more "commonsense" notion of it as a desirable state of character. For Melden, the talk of dignity divorced from all concrete human interests and as much a characteristic of "scoundrels, fools and, docile slaves" as of the wise and self-possessed is deeply at odds with the ways the term is employed in ordinary language. (p. 33) More important, it makes it impossible to say concretely what social and political conditions dignity requires. Rather than dignity grounding rights, Melden concludes that rights are useful in enabling people to live dignified lives as the right to die enables the terminal patient to end her life with dignity intact. Only saints or heroes, Melden reminds us, maintain their dignity in the face of the oppression and adversity likely to be faced by rightless persons.

The essays in *A Culture of Rights* tend to be synthetic, aimed at orienting the reader to the state of rights discourse at the founding and at present in political and legal thought. They are of universally high quality though Knud Haakonssen's piece stands out as a *tour de force*. Haakonssen explores European natural law theories and the relation between them and early American jurisprudence. He shows that the debate over rights in the founding era occurred within the context of a much richer moral philosophical universe than is commonly supposed. He sketches two broad European theories of rights, the first being a subjectivist account found in Grotius and Hobbes within which rights are taken to be fundamental moral facts about persons from which we can derive certain "natural law" duties as, for example, to endeavor peace. The second and more prevalent theory found in Pufendorf, Locke and others derives rights from some higher moral standard. In this view, "rights are not simply powers granted, but powers granted for a purpose . . . namely, that of contributing to an overall moral order." (p. 36) Rights are powers rational agents require so that they can carry out their duties under natural law. The American founding era, Haakonssen argues, witnessed a rising tension between these views and, at best, a partial reconciliation. His argument should add considerably to the sophistication of discussions of rights in the founding era which too often resort to simple and false dichotomies as between rights and virtues or duties, or between private rights and common interests.

Readers will also find useful both William Galston's essay which surveys the status of rights in contemporary political discourse, particularly in the 'liberal-communtarian debate, and William Fisher's discussion of modern American legal theory implicated in interpretations of the Bill of Rights. Arguably, the talents of these authors could have been better used had they been encouraged to present original contributions rather than syntheses of the state of the art in their respective professions. Galston drops some provocative hints regarding the inadequacies of contemporary liberal rights talk and how liberals must better articulate not only what rights we have but how it is right to use them. Nonetheless, these remain little more than hints and readers will have to search elsewhere in his opus for more conclusive discussions.

Finally, Alan Ryan's essay on the nature of rights talk in Great Britain as compared to the United States bears mention as much for the ambiguities it shows as for the conclusions it draws. The British, Ryan reminds us, tend to believe that rights are protected first and foremost by a liberal political culture rather than by mere "parchment barriers." This common culture has left them reasonably secure in their rights even given party dictatorship in parliament and a rather passive, state-supporting judiciary. Moreover, it has allowed for less conflictual handling of issues like abortion which, in the United States, becomes reduced to the assertion of incompatible rights claims.

Yet, Ryan is conflicted when considering whether Great Britain should adopt a formal bill of rights though he finally supports its doing so with great reluctance. At times he seems to believe that parchment is too flimsy a barrier to defend rights strongly enough. Elsewhere, he is concerned that Great Britain does not become the rights-obsessed political culture he sees in the United States and fears that a bill of rights could lead to this end. Interestingly, this is just where we came in. As the essays by Rakove and Hutson amply demonstrate, Ryan's conflict was very much Madison's as well even as he drafted our Bill of Rights. These writers, expressing a strong streak of political prudence some two hundred years apart, seem to suggest that even in the case of rights, one can have too much of a good thing.

*University of California, Davis* RICHARD C. SINOPOLI

**Liberalism, Citizenship and Autonomy.** Edited by David Milligan and William Watts Miller. Brookfield: Ashgate Publishing, 1992. 228p. $59.95.

This volume of 13 essays is informed largely by the "liberalism–communitarianism" debate as instigated by such writers as Michael Sandel and Alistaire MacIntyre. The opening one, "Strangers and Brothers: Liberalism, Socialism and the Concept of Autonomy," is Susan Mendus's Colston Lecture at the University of Bristol in 1990. Then there are four on citizenship (Richard Norman, Martin Hollis, Anthony Skillen, and William Watts Miller); four on liberalism and rights (Hillel Steiner, Richard Bellamy, Raymond Plant, and John Skorupski); and four on autonomy (David Archard, Andrew Mason, Jean Grimshaw, and Onora O'Neill). Each has its own arguments and its own morals to draw; but it would perhaps be fair to say that for the most part, the thrust is toward upholding the late twentieth-century liberal welfare state, with one or two aspiring somewhat farther toward socialism and not a single one proposing to defend what is nowadays characterized as right-wing, that is, fully classical liberalism (or libertarianism). As the writer of the editorial introduction, William Watts Miller puts it, "They also tend to approach these questions by challenging certain forms of liberalism—deontological, contractarian, economistic—that have had a recent comeback." Not a one, however, actually represents that supposed "comeback" or, so far as I can see, has a very good idea what its abler representatives are saying these days. So there is not really a lot of major disagreement among the contributors, and much of the argument is aimed at men largely of straw—a pity, for it would have livened things up. Even so, there is plenty to engage the mind in this collection of literate and densely reasoned essays. And there are differences among them as to how to conceive the foundations of the liberal welfare state.

A major concern of the authors is this general dichotomy: on the one hand, an atomizing liberalism whose premise is that we are all strangers, by virtue of being totally autonomous; on the other, a suffocating socialism in which all are presumed to be brothers, bonded together by virtue of unchosen relationships. The autonomist is said to hold that all obligations and all ultimate ends are *chosen* by those who espouse them, whereas an opposite view has it that individuals are constituted entirely by the sum of their community relationships. Several of the authors struggle to a workable "middle ground" between these views.

It seems to me that we also need to consider the possibility that this is the wrong set of questions altogether. For one thing, it is at least worth mentioning that the premises allegedly held by the opposite parties are obviously false. It is false that I *chose* to fall in love with the particular people I love, that I *chose* to be interested in philosophy, and so on. Yet it is also obviously false that we are all siblings, or even cousins, or friends either under or above the skin; and there is no point in wistfully supposing that maybe we are after all. It is false that all siblings, cousins, and friends *must* acknowledge all those obligations proclaimed by enthusiasts for brotherhood. People have, after all, disowned their own children; Cain did not shrink from slaughtering Abel; and so on. On the other hand, people have voluntarily enlisted for ends not thrust upon them by any antecedent ties to anyone in particular and have nevertheless held to those ends so strongly as to sacrifice their lives to them.

It is also obvious, on small reflection, that what we are related to by virtue of being siblings or fellows in community or anything of the sort are *particular* groups of people, groups that may, and often do, have little in common and who are as like as not to be found at each other's throats (consider contemporary Yugoslavia) and to be at them by virtue of that very brotherly love that the one sort of "liberal" appears to want to hold up as the very model for moral and political obligation. This does not seem to have made any of the authors draw back and consider that perhaps the contractarian model, in which we are *not* assumed to have any such antecedent ties, is and must be the appropriate one for adjusting relations in our actual world, which is, frankly, largely a world of strangers.

There are some thought-provoking essays here. I was struck by John Skorupski's probing "Liberal Elitism," and there is a brilliant suggestion about how to construe autonomy in relation to community in the paper by Andrew Mason. The reconstruction of Kantian autonomy by Onora O'Neill is a major contribution, as well. Hillel Steiner traces the argument from Locke's idea of ownership by labor in two opposite directions toward libertarianism and socialism (I think the libertarian one wins hands-down but cannot indulge in refutation here). But all are worth reading from one or another point of view.

*University of Waterloo* JAN NARVESON

**The Ennobling of Democracy: The Challenge of the Postmodern Age.** By Thomas L. Pangle. Baltimore: Johns Hopkins University Press, 1992. 223p. $25.95.

Pangle has written a deeply thoughtful book about the theory of republican regimes, which in their modern form have come to be known as democracies. The supreme virtue of the book is its seriousness. Pangle is in no doubt that it matters fundamentally how we think about the purposes and workings of régimes in which the people are to govern. If our theorizing is loose, banal, and evasive (as much of it, in fact, is), Pangle believes that we will be unable to diagnose the failings of modern republican regimes and be unsure how to remedy them.

At the center of what is going wrong in modern republican regimes, says Pangle, is a spiritual climate characterized by a pervasive disbelief in the philosophic pillars of modernity. The book begins with a discussion of the attack on modern Enlightenment philosophy that has come to be known as postmodernism, an attack also aimed at dismantling the claims of the older Socratic rationality. Pangle's purpose in the book is partly to defend the claim of modern theorists of republicanism that they have created if not a full science of free government, at least something worthy of respect. Even more importantly, he wishes to show how Socratic rationality (as it finds its expression in philosophy, particularly the dialectic questioning and answering that rises to the most fundamental questions) is necessary to a diagnosis of the shortcomings of contemporary republican regimes.

Not only, then, are the postmoderns wrong in fundamental ways, contends Pangle; but the republicanism of the Enlightenment needs classical civic rationalism. There is a practical compromise possible between them, says Pangle; and at the deepest level, he wishes to engage the reader in the possibility that modern rationality must be subordinated to the classical conception, since modern rationality itself is unable to provide a compelling account of the human condition, one grounded in an understanding of what kinds of beings human beings are.

The chief problems of modern republicanism seem to be two, according to Pangle. First, it is increasingly unable to give a compelling defense of its central doctrine, namely, that there are natural rights whose fostering is the chief object of political rule. Natural rights—and all other claims to transcendent political truths—have come under withering attack from those who take their intellectual bearings from Heidegger and Nietzsche, whom Pangle and others collectively dub postmodernists. Postmodernists argue that there are no secure evaluative foundations for judging political life, only the privileged discourses of those who have been able to enforce their particular conception of the world against those too weak to resist.

Pangle believes that such postmodernist attacks must be taken seriously; hence he gives careful and respectful attention to the thought of Jean-Francois Lyotard, whose work, he believes, is the most thoughtful in the postmodernist cannon. Among other things, Pangle is struck by how weak the attempts to respond to criticisms like Lyotard's are. When the defenders of modernity invoke a bit of Kant to shore up the cause of rights (as they typically do), Pangle contends that they seem not to have understood him, since if they did, they would find him much less attractive than they suppose. It is here, in the most fundamental defense of the modern project, that modern republicanism needs classical rationality, since it is only by contending with Heidegger, who is the generative source of postmodernism, on his own grounds, that is, in his critique of classical rationalism, that postmodernism itself can be combatted. Unless Heidegger can be kept at bay, the postmodern critique of rights and other conceptions of the political right and good will prove decisive.

Pangle is here very tactful in not directly addressing whether this subordination of modern rationalism to classical rationalism will issue in any sustained defense of human rights. Perhaps he believes that since the kind of aestheticized politics that postmodernism seems to have a penchant for is likely to be so odious, we ought to take our chances with classical rationality even if the outcome is only a defense of rights on the surface, with our deepest political commitments being elsewhere. Maybe supporters of human rights will have to settle for what a discreet revivified classical rationality can deliver.

The second fundamental problem of modern republicanism also points to its rethinking in light of classical civic rationality. Pangle notes what others have also seen, namely, that modern republican theorists have felt unable to build into the régime adequate moral and educational foundations. It is not that they were unaware of the need to do so, Locke being a conspicuous example of a theorist who gave extended attention to the problem of education, as did members of the American founding generation. But they did not (perhaps because they could not) foresee that the attempt to school the passions of the new citizens in an enlightened self-interest would ultimately contribute to an undercutting of a politics devoted to fostering individual liberty. Enlightened self-interest is not enough, thinks Pangle.

The shortcomings for Pangle of the modern republican theory of education seem to lie both in a too-restricted conception of its content and in its increasing inability to defend even the modest proposition that education in a republic must find a home for the celebration of freedom, the defense of rights, and the honoring of those who devote their lives to the cause of free government. The points come together in Pangle's argument that higher education in a modern republic must give pride of place to the exploration of Socratic rationality because only that rationality can provide a defense against what he terms the philistine relativism that threatens the capacity to define any principled basis for political life. Without a defense against such relativism—especially in its postmodern form—the civic education necessary for modern republicanism cannot be defended, says Pangle. Education in a modern republic must have room for a teaching that gets citizens to look beyond their self-interest to an appreciation of the principled bases of the régime and to an understanding of what is required by way of political activity to give those principles life.

Four questions are prompted by Pangle's arguments. First, must what Pangle terms "collective Hobbesian shrewdness" issue in an account of the purposes of modern republicanism as the pursuit of a "painless, peaceful, prosperous creature-comfort" (p. 39); that is, must we aspire to so deeply a misleading and perhaps dangerous banality? Perhaps a little Hobbes mixed with a little Old Testament might lead to a less juvenile view and (maybe even more importantly) to a set of political practices in the service of individual liberty that are possible to sustain over time. In any event, the American experiment is just that, an experiment in the marriage of Hobbes and the Bible. Second, and closely related, there is in Pangle's argument a kind of unseemly haste in moving from the signs of relativism among intellectual elites to a renewed and expanded role for philosophy in the classical sense. A debased republican citizenry cannot hear philosophers, nor (more or less by definition) can postmodern intellectuals. Is it possible that only republican political practice can find the kind of intellectual resources that can repair its own failures?

Third, while Pangle may be right that American intellectuals are awash in the moral miasma of a foreign relativism, it is possible that he underestimates the extent to which the rubes are suspicious of city slickers. After all, there is still a great god-fearing middle class who seem untempted by the new intellectual wares. While those whose line of work is other than in the academic culture industry may indeed be deeply uncertain about how to conduct their lives, they seem not to be open to the kind of relativistic leveling that so worries Pangle. There seem plenty of Americans ready to follow a natural aristocracy if only one would announce itself. Finally, as with all arguments of the kind Pangle presents, there is the worrying thought that something fundamental is being left out. Thus, how *much* leveling can a republican regime live with? Or, for that matter, how much material inequality will bring it to its knees? It is too easy to leave out the hard comparative empirical analysis when one thinks that the republic is in deep danger. But is it, really? Alas, there is no substitute for finding out.

*University of Maryland* STEPHEN L. ELKIN

**Republics Ancient and Modern: Classical Republicanism and the American Revolution.** By Paul A. Rahe. Chapel Hill: University of North Carolina Press, 1992. 1,201p. $49.95.

In *Natural Right and History* (1950), Leo Strauss argues that modern political theory involves a deliberate lowering of goals. Paul Rahe's *Republics Ancient and Modern* is an extended and erudite elaboration of that thesis. Weighing in at 15 pounds, the book brings to mind the reviewer of *Gone with the Wind*, who commented that he had heard of books you couldn't put down but had never before seen one you couldn't pick up.

In the opening and relatively short section, "The Ancien Regime," Rahe makes a strong and convincing case for the utter otherness of the ancient world. The classical and modern variants of republicanism with which Rahe's book deals are not different—they are polar opposites. Where the ancients put the highest premium upon honor and glory and bent every civic effort to the achievement of communial solidarity, moderns extolled the individual and conceived of politics as a way of facilitating man's well-being, what Hobbes called commodious living. In Rahe's account, wealth levelling, pederasty, poetry, and the suppression of family loyalties became elements in the all-encompassing public education of classical Greece. Preferring war to peace and theory to practice, the ancients prized civic virtue, human excellence, and immortality far more than self-preservation, that cornerstone of modern political theory. So great was Archimedes' love of theoretical mathematics, Rahe reminds us, that on the day Romans sacked Syracuse he was pondering a geometric form, oblivious to his and the city's impending destruction (p. 100).

Contemplating the diversity of opinions that led to majority faction, James Madison in *Federalist 10* had considered the options of removing the causes of this diversity or controlling the effects. A civil libertarian, Madison dismissed the former as a cure worse than the disease. The ancients, Rahe stresses, would have allowed neither differences of wealth nor religious heterodoxy to divide the citizens. Indeed their comprehensive education of citizens aimed at nothing less than giving all the same opinions, passions, and interests, as Madison himself acknowledged (pp. 59–60). For their part, the moderns invented humanity—celebrating equality, self-improvement, tolerance, and (with the passage of time) free inquiry (pp. 259, 279). In Franklin's list of the 13 virtues that he practiced in rotation each week, "piety, liberality, magnanimity magnificence, and courage" are conspicuous for their absence (p. 331).

Needless to say, commerce and technology scarcely figured in classical estimations of valued activities. Where Montesquieu and his contemporaries treated the invention of the bill of exchange as a turning point in history (p. 61), the Greeks considered the very ends of modern banking a vice. Instrumental and private, rather than substantive and public, the moderns not only lowered their goals, they came to doubt the possibility of the nobility the Greeks cherished. As Lord Halifax observed of the person who aspired to godlike virtue, "by aiming to be more than a man, he becomes a Beast" (p. 273).

It is also part of Rahe's understanding of the modern revolution in sensibilities that the outspoken criticism of classical values found in Machiavelli, Montaigne, Bacon, Locke, and Hobbes was, as often as not, a covert criticism of Christian orthodoxy. The Christians, he stresses, had learned to emulate the Greeks in their renunciation of progress and material comforts. Therefore, the heralds of modern this-worldliness attacked the original to get at the copy. This displaced criticism was also much safer for men discoursing on politics in the age of absolutism. In elaborating this point, Rahe explicitly summons Strauss for his treatment of the dissembling that marks the writing of great, régime-threatening political theorists (p. 918).

To explicate in so thorough a manner the diverging ethical paths of classical and modern republicanism, as Rahe does, is to take on J. G. A. Pocock and his associates in the "republican synthesis" that has dominated scholarship on the founding of the American nation. Rahe challenges three of their contentions: that modern republicanism was anticapitalist, that Locke took a back set to Machiavelli and Montesquieu, and that American liberalism sprang from a postrevolutionary failure to sustain civic virtue. The burden of his argument with prevailing historiography is confined to the book's notes, while the text carries a careful assessment of the critical decisions made by the statesmen who brought into being the *novus ordo seclorum*.

In his *Ancient Constitution* (1957), Pocock described the paradox of Renaissance scholars' studying the mores of antiquity with unbounded zeal in their eagerness to emulate them only to discover how different classical civilization was. This, in Pocock's view, represented a confrontation with change over time and made way for the historical consciousness of the seventeenth century that eventuated in Hegel's historicism. Rahe's study pivots on the same incommensurability of classical and modern worlds, but he totally rejects the implications of this encounter with historicity. Indeed, he chastises contemporary scholars repeatedly for their lamentable acceptance of the social construction of reality. (Postmodernism figures not at all.)

Despite its length, *Republics Ancient and Modern* is more an expansive essay on modern and classical conceptions of human nature than a history of the régimes or an analysis of their structures. The author is contentious in the good sense of the word; his voice is authoritative, and "awesome" is the only way to characterize his grasp of the conversation about politics that has been running now for close to two-and-a-half millennia. There is scarce a speaker who has escaped his notice, as the 70-page index shows. Yet Rahe's disdain for the very issues that divide him from others prevents his engaging with the conversation of his contemporaries, most of whom are trying to figure out how political languages take shape, what bearing they have on action, and how they interact with circumstances and contingencies. By making his argument through assertion and evidence, rather than reasoning with those he contradicts, Rahe fails to open up a world of meaning for his readers and leaves us, instead, in possession of a collection of finely honed opinions.

*University of California, Los Angeles* JOYCE APPLEBY

**On Heidegger's Nazism and Philosophy.** By Tom Rockmore. Berkeley: University of California Press, 1992. 382p. $45.00.

Forty years ago, the name of Martin Heidegger was scarcely to be heard in English-speaking academic circles. It is a story of considerable interest, not the least with respect to what one could call "the inner destiny" of analytical philosophy, that Heidegger is today arguably one of the two most influential philosophers in the United States, the other of course being Nietzsche. I use the term "influential" with respect to the academic and intellectual scene as a whole, not as restricted to professional philosophers.

To give only the most important example, Heidegger's interpretation of Nietzsche underlies virtually all branches of advanced French thought since the 1960s. This interpretation transforms Nietzsche's prophetic and (in a sense that he himself relates to Plato) political teaching into an ontology of radical chaos. Heidegger's own earlier interest in ontology gradually devolved into a celebration of the spontaneous manifestation of Being as veiled in the contours of a historical epoch, superficially similar to process philosophy as seen from the standpoint of the Neoplatonist doctrine of the emanation of Being from a hidden source. In the course of this devolution, Heidegger's earlier political doctrine—ostensibly derived in large measure from Hölderlin, in which the identification of the German *Volk* as the authentic spiritual people was all too easily wedded to the vulgar street slogans of National Socialism—itself gave way to an invocation to "releasement" (*Gelassenheit*), that is, to the claim that the thinker is subordinated to the "errance" of Being (and hence can take no personal responsibility for his own political errors).

It has long been suggested that the shift from authentic resolve to the resigned attentiveness to a possible new "gift of Being" or epochal *E-vent* was not unrelated to Heidegger's realization of the terrible mistake of his association, both as member and ideological interpreter, with the Nazi party. The question of the actual events of Heidegger's membership in the Nazi party, which we now know to have continued long after his resignation from the post of rector of Freiburg University, as well as of his personal attitude toward the Jews, has been a matter of debate for decades. This debate has from time to time been punctuated by careful studies of the political implications of Heidegger's philosophical doctrines. Until fairly recently, these studies were produced largely by continental thinkers. The tide, however, has begun to shift; and Rockmore's book is a useful—in many ways a distinguished—contribution by an American scholar to the analysis of the inner connection between Heidegger's nazism and his philosophy. Rockmore traces the evolution of that connection from the time of the notorious *Rektoratsrede* through the lectures on Hölderlin and Nietzsche, the interpretation of technology as the last epoch of metaphysics, and the *Beiträge*—only recently published but long famous among Heideggerians as the ostensible document of record for Heidegger's esoteric teaching. There is a careful chapter on the French reception of Heidegger's nazism and a brief conclusion in which Rockmore makes the decisive point that Heidegger sacrifices human being to Being (p. 290), a point that I would sharpen by saying that Heidegger's doctrine sacrifices beings altogether to Being.

The book falls into two parts, consisting of the first three and last five chapters, respectively. In the first part, Rockmore establishes a series of provisional conclusions (summarized on p. 123), which are then confirmed by the textual and documentary analyses of the second part. His investigation allows him to affirm that Heidegger's "theory of Being" (not a felicitous expression) leads necessarily "to a totalitarian political movement" (ibid.). Rockmore also argues that Heidegger, in effect, lied about the degree and duration of his involvement with the Nazis, as well as about the motives that led him to assume the rectorship of Freiburg University in 1933. I have no space here to discuss in detail Rockmore's findings. In general, he proceeds judiciously and with a detailed grasp of the texts upon which he rests his case. My major objection to his study can be illustrated with his establishing the undemocratic and totalitarian nature of Heidegger's thought as follows: "Heidegger's ontology and politics are intrinsically and inseparably linked through his conception of authenticity, his lifelong quasi-Platonic understanding of the political vocation of the thinker of Being, and his commitment to the destiny of the German *Volk*, all aspects of his understanding of Being" (p. 284). I believe that this is mistaken, and to a considerable extent refuted by Rockmore's own analysis of the historical development of Heidegger's views. Rockmore's conclusion is more accurate as an account of the early Heidegger's philosophical writings. I do not doubt that Heidegger continued in subsequent years to believe in his political vocation and the destiny of the German *Volk*, but one would be hard pressed to deduce this rather traditional German conservatism from the *Beiträge* and the awaiting of the *E-vent*. I add, finally, that Rockmore exhibits throughout a superficial appreciation of "Platonism," best illustrated by his failure to distinguish between philosophical rule in the name of the Good and self-aggrandizement in the name of authenticity. Rockmore thus repeats the error of Heidegger, as well as of such Marxist counterparts to Heidegger as Lukacs and (at least with respect to rhetoric) Kojève, in failing to draw the appropriate lesson from the publication of Plato's Seventh Letter, in which the philosopher recounts the disastrous consequences of his journey to Sicily. The book would have been still more valuable if the author had thought more deeply about the relation between philosophy and politics; but as it stands, it is a serious contribution to the English-speaking debate about Heidegger.

*Pennsylvania State University* STANLEY ROSEN

**Fear of Diversity: The Birth of Political Science in Ancient Greek Thought.** By Arlene W. Saxonhouse. Chicago: University of Chicago Press, 1992. 253p. $29.95.

**Multiculturalism and "The Politics of Recognition."** By Charles Taylor. With Commentary by Amy Gutmann, editor, Steven C. Rockefeller, Michael Walzer, Susan Wolf. Princeton: Princeton University Press, 1992. 112p. $14.95.

In his typically pithy way, Freud once commented that a person who could not pull himself or herself together to "form a judgment" was little better than a doormat. Taylor and Saxonhouse display our human capacity for judgment at its most generous. Taylor's essay, with accompanying commentary, and Saxonhouse's fine treatment of Greek philosophy, tragedy, and, with Aristotle, invention of a specifically political science, revolve around perennial themes that ongoingly inspire and vex us. In her introduction to Taylor's work, based upon his Inaugural Lecture for the University Center for Human Values at Princeton University, Amy Gutmann claims that Taylor "steps back from the political controversies that rage over nationalism, feminism, and multiculturalism to offer a historically informed, philosophical perspective on what is at stake in the demand made by many people for recognition of their particular identities by public institutions" (p. 6). In what might be called Taylorian fashion, Taylor weaves his way through competing and conflicting claims to recognition, offering up a dialogical ideal by contrasting competing monologisms.

His argument holds that those who wish to defend a strong vision of unity or common identity and purpose and, ironically no doubt, those who wish to push a strong vision of difference and antagonistic identity, wind up with a monological ideal and claim. Either all differences must be quite overcome and transcended in a Rousseauian move towards unity, or differences take on the status of incommensurable identities—as some radical 'difference' theorists and political activists would have it. In either case, nobody, finally, has to engage anybody else. In the Rousseauian case, there is nothing to engage as a common purpose and unity prevails. In the strong claim to different identities, human beings inhabit separate worlds (based on gender, ethnicity, sexual identity, and so on) that make authentic engagement with the 'Other' impossible for that Other can be seen only as a looming hegemon threatening to engulf and efface the marks of one's difference.

Taylor, by contrast, unpacks the ways in which "my discovering my own identity doesn't mean that I work it out in isolation, but that I negotiate it through dialogue, partly overt, partly internal, with others" (p. 34). My own identity always depends on dialogic relations, whether or not I acknowledge this crucial fact. The dialogic point holds for every sphere, from the most intimate to the most public. Taylor insists that a politics of equal respect worthy of endorsement is one that respects our human dignity and, for that reason, steps back from either obliterating or absolutizing the distinctive marks of our identity. Critical of past and present attempts to forge homogeneous, collective unities, Taylor is equally forceful in pointing to a "severe problem with much of the politics of multiculturalism," namely, a "peremptory demand for favorable judgments of worth" which are themselves homogenizing. For by "implicitly invoking our standards to judge all civilizations and cultures, the politics of difference can end up making everyone the same" (p. 71).

Saxonhouse, one of our foremost interpretors of classical Greek tragedy and philosophy, touches on precisley the same themes in her treatment of "fear of diversity" in ancient Greek thought. Beginning with a wonderful meditation on Aristophanes' *Ecclesiazusae*, Saxonhouse shows that this comedy of reversal—in which differences between the sexes, the old and the young, the city and the family, the good and the bad, the beautiful and the ugly are effaced—winds up introducing chaos precisely *because* that is where a "drive for unity" inevitably take us (p. 1). Within the play, a unity that ignores all differences does not work and, Aristophanes suggest (and Saxonhouse seconds the motion), cannot work. In losing all that is distinctive and separate, authentic sharing (to hold in common) is itself foresworn. Moving on to show the ways in which pre-Socratic philosophers articulated an "epistemological victory of the mind," Saxonhouse displays the tragedian's rejoinder, Plato's complex restatement of the unity theme, and Aristotle's robust discovery of an authentically political science.

Her story reminds us that the Greek city of adult males, drawn to a unified vision of the city grounded in the myth of authochthony, or birth from the earth by contrast to birth from the body of a woman, were nevertheless compelled to confront powerful portrayals of women in the tragedies—women whose lives and words show the tragic consequences of attempts to transcend the female, hence to ignore her as a "constant reminder of the diversity out of which the world was made and as a constant warning against the attempt to see the world as a uniform whole and, therefore, subject to simple answers and rational control" (p. 53). Nowhere is this quest for unity more powerfully argued than in Socrates' Callipolis, his "city in speech," in which all women of the guardian class are in common for the men, and living together in private is, as Saxonhouse reminds us, for no one. "Here we see the true destruction of the female and her elimination from Callipolis as she dissolves into the male and the family dissolves into the city" (p. 151). Saxonhouse concludes that this heroic city has a "rather deathlike quality"—there is no creativity, no particular identities, male and female are obliterated, and a "masculine model of rational omnipotence has reigned to create a vision of monistic simplicity . . ." (p. 157). Saxonhouse might have discussed the irony of (some) feminist attraction to this vision of the obliteration of all marks of difference, *especially* those to do with women, but she does not, preferring to stay internal to the texts she is interpreting rather than to engage various interpreters, whether or not feminist.

Arriving at Aristotle, Saxonhouse suggests that politics is made necessary by our differences and takes place as we engage those differences in our search for those commonalities that make a way of life together possible. The way of life she (and Aristotle, on her reading) have in mind is "composed of a multiplicity of parts" and based on a "balancing of different claims" (p. 187). It is a world of tension, conflict, and competition which cannot, finally, be overcome except at a price none but the most determinably utopian and willful—and, finally, anti-democratic—will be prepared to pay.

*Vanderbilt University* JEAN BETHKE ELSHTAIN

**Essays on Henry Sidgwick.** Edited by Bart Schultz. New York: Cambridge University Press, 1992. 421p. $65.00.

Henry Sidgwick's life was, even for a nineteenth-century don, tranquil. A propensity to asthmatic attacks kept him in Cambridge—away from the Alps, revolutions, and America—even in the vacations; and a diffidence that may have resulted from early homosexual attachments, nearly all traces of which were removed from his correspondence by his wife and brother (also homosexual), kept him from a political or journalistic career. To Keynes's more daring generation he appeared timid; and although his preoccupation with philosophical definitions seemed to concur with the later dominance of linguistic analysis among the logical positivists, this was by no means a promising sign, given the aridity of that discourse. To Perry Anderson in *Components of the National Culture* (1968), he was a "grey and ponderous figure" foredoomed to be Arthur Balfour's brother-in-law when he ought to have been pioneering sociology like Weber and Durkheim.

The book of essays on Sidgwick edited by Bart Schultz had its origins in the first symposium devoted to the philosopher. Divided into four thematic sections—"Common-sense Morality, Deontology, Utilitarianism," "Egoism, Dualism, Identity,""Hedonism, Good, Perfection," and "History, Politics, Pragmatism"—it ranges across the breadth of his interests from metaphysics to economics and politics, containing en route much of interest for the political scientists. Stefan Collini and James T. Kloppenberg have contributed chapters on Sidgwick's political analysis and his relation to American pragmatism, respectively. Collini is elegant and persuasive but somehow makes him out to be both a duller and a more conservative author than he, in fact, was: D. G. Ritchie's "Benthamism grown tame and sleek." He observes that Sidgwick as essayist and correspondent was more adventurous, but does not follow this up by using Sidgwick's very radical essay, "the Theory of Classical Education" of 1868, demanding an end to the dominance of classics, his address on authority to the Cambridge Reform Club in 1872, or the fascinating exchange of correspondence with Sir Lewis Mallet in 1887, which ended up with Cobden's acolyte accusing the active Liberal Unionist of near communism. Sidgwick's British Association address of 1886, "Economic Socialism," is surely germane to Kloppenberg's case for strong affinities between Sidgwick's "utilitarianism founded on intuition" and the piecemeal social engineering advocated by John Dewey and William James. Believing that there was "a general movement towards socialism in the modern civilised community," what Sidgwick feared was not the threat to laissez-faire but the abandonment of logical political direction in favour of the dutch auction of party competition. His relation to the Balfours, a brisk and unsentimental lot, did not convince him that the conservatives were any more reliable.

William K. Frankena on Sidgwick and moral dualism, and J. L. Mackie on Sidgwick's moral pessimism, are also suggestive here, as they indicate his admiration for—though not belief in—the Scottish philosophy of "common sense" (generally regarded as destroyed by J. S. Mill's *Examination of Hamilton* in 1865) as providing a practical and robust means of mediating between enlightened self-interest and moral duty. Unable to accept the theistic postulates that underlay Thomas Reid and Dugald Stewart's notion of "consciousness," Sidgwick found that "the Cosmos of Duty is indeed a Chaos" (as Mackie puts it, significantly borrowing from Tennyson). No matter how resonant, a poetic symbol could not resolve his dilemmas over the reasons for right action. These suggested further fields for professional philosophers; but they do not seem to have detained his more energetic contemporaries James Bryce, James Stuart, and Eleanor Balfour (Sidgwick's wife) for long. The practical attention to social and political questions that Perry Anderson misses was to be found in Sidgwick's obvious utility to these commonsensical reformers—all Scots, intriguingly enough—as someone who clarified the language and arguments that they intuitively grasped at. Unlike A. V. Dicey, whose lumpen-Benthamite dogmatism is still rotting British political discourse, Sidgwick's "impartial reflection on current opinion" bore fruit, collectively, in the remarkable social studies and educational reforms of his age, from Bryce's *American Commonwealth* to the movement for women's higher education. With commonsense, pragmatism and cautious collectivism again on the agenda, we have not heard the last of him.

*Universität Tübingen* CHRISTOPHER HARVIE

**Inequality Reexamined.** By Amartya Sen. Cambridge: Harvard University Press, 1992. 207p. $29.95.

Amartya Sen is a distinguished economist and philosopher, probably the world's leading figure straddling these fields. In recent years one of his notable projects has been to apply the analytic techniques of economic theory and contemporary philosophy to the clarification of fundamental problems in normative political theory. *Inequality Reexamined* summarizes and further develops recent work by Sen on the political ideals of equality and freedom. There is more summary here than new development: those familiar with Sen's prior writings on the topic will find little in this book that is unfamiliar. But it is useful to have Sen's ideas (heretofore scattered through many essays) collected and organized in this monograph-length essay. Sen brings a hard-edged intellect to regions of thought usually regarded as slushy and amorphous. The results are impressive.

Political philosophers tend to treat "equality" and "liberty" as counterposed values and to see socialists and radicals as favoring the former, liberals and libertarians the latter. Sen finds this way of thinking confused. According to Sen, any normative political theory worth taking seriously in the contemporary world will be egalitarian in some sense. Given the belief in the fundamental equal worth of each person that is deeply entrenched in modern moral culture, we would not countenance a political morality that failed to prescribe a foundational norm of equal treatment of all persons. Liberals, libertarians, utilitarians, socialists, conservatives, communitarians, feminists, and other are better taken to be differing on the important question, Equality of what? In what ways should people be treated the same? As Sen notes, even such libertarians as Robert Nozick, who might be thought to be hostile to the value of equality, propose that all persons equally possess the same basic individual rights, which should be respected. Sen does not claim that one ideal of equality should take precedence over all others. Perhaps different norms should govern in different contexts. This book meticulously and imaginatively develops an ideal of equality of individual positive freedom and cautiously urges its merits.

Sen explores the idea of identifying freedom with what he calls "capabilities to function." We can identify valuable ways of being and doing, such as being well nourished, being literate, playing tennis at a specified level of competence, solving significant mathematical puzzles, taking a holiday in Paris, and so on. Call these "functionings." For each dimension of functioning thus identified, we can define a scale and establish the extent to which an individual achieves or gains this functioning. If we have specified all the dimensions of functioning that have a bearing on the issue of whether the person lives well or badly, we can identify the vector of the person's functioning scores with that person's well-being. In the same vein, the person's capability to function can be identified with the set of all the possible combinations of functionings that person could achieve. Sen writes, "Capability is, thus, a set of vectors of functionings, reflecting the person's freedom to lead one type of life or another" (p. 40). Sen further distinguishes this notion of *well-being freedom* from the wider notion of *agency freedom*, which is greater or lesser depending on the extent to which the agent is able to achieve significant life goals regardless of whether the achievement would contribute to well-being. To borrow Sen's illustrative example, imagine that a person is sitting on a river bank enjoying a picnic lunch at the time that a stranger falls into the river and is threatened with drowning. Now add to this story the further detail that the individual is able to jump in the river at considerable personal cost and save the drowning stranger. With this added detail the individual's agency freedom increases (she is able to fulfill the goal of saving lives, which takes priority for her in these circumstances), while the individual's well-being freedom simultaneously decreases. (Given that now she can save a life at considerable personal cost, the tranquil features of the picnic that might have contributed to her well-being have been lost, regardless what choice is made.)

Sen makes a plausible case that when we are assessing people's well-being for purposes of considering their situation from the standpoint of distributive justice, we should be concerned with the actual vector of functionings achieved, rather than the pleasure or happiness or subjective preference satisfaction level that the individual attains. For example, we might well be concerned with the reduced functionings that blindness or crippled legs or mental retardation induces, quite independently of whether or not the individual who suffers form one of these afflictions is able to adjust to it so that no loss is registered in pleasure or happiness or subjective preference satisfaction. (Of course, each of these commonly proposed well-being measures would qualify as one dimension of functioning among others.) In a parallel fashion, Sen makes a plausible case that when we are assessing people's freedom to lead lives they value, the best measure of real freedom is capability to function. Along this line, Sen urges that principles of justice should be responsive to a wide variety of types of information about people's condition corresponding to the wide variety of our actual concerns about people's condition. According to Sen, the paradigm of a reductive principle prescribing a far too narrow informational basis for social policy judgments is utilitarianism.

Sen uses the capability approach to freedom and to distributive justice to criticize John Rawls's *A Theory of Justice* (1971). A central tenet of Rawls's theory is that interpersonal comparisons for a theory of justice should be made in terms of people's shares of primary social goods (goods distributable by society that rational persons will want more of, rather than less of, whatever else they want). Primary goods are multipurpose resources, means to many goals. Sen's objection, which strikes me as cogent, is that it is fetishistic for a theory of justice to assess people's condition in terms of the resources they possess. What should matter to the theorist is what people's resource shares enable them to be and do. Attention should focus not on the means to freedom that people have but on the extent of freedom they "actually enjoy to choose the lives that they have reason to value" (p. 81). Since people differ significantly in their abilities to transform given resources into desirable functionings, the switch from a resource-based, to a capability-based, approach to justice would lead otherwise similar theories to generate very different recommendations for policy.

Regarding the capability approach, the question arises whether interpersonal comparison is feasible unless we have some evaluative basis for singling out a reduced number of functionings that are truly important or desirable. After all, even the poorest person will be able to reach countless functionings (e.g., quick access to dirty drinking water in a polluted stream, the camaraderie of impoverished neighbors) that will be beyond the reach of a well-off person, who will, of course, have access to countless functionings that are quite beyond the reach of the poorest person. A threat of noncomparability looms. On Sen's preferred-capability approach, what, if anything, allows us to rank persons as overall better or worse off on a "well-being freedom" scale? Sen has two responses to this challenge. On the one hand, he has interesting suggestions about ways to achieve partial comparability. On the other hand, he cautions against overweening ambition on the part of theorists of justice: the best understanding of the concepts of freedom and equality may require us to countenance significant incommensurability.

Sen flirts with, but does not decisively embrace, the idea that we should select the functionings that matter according to a perfectionist standard of worthy human life. His formulation identifies the extent of persons' well-being freedom with the extent to which they enjoy freedom to chooses lives "that they have reason to value" (p. 81). This suggests that the addition of options that are not objectively worthwhile does not contribute to the person's freedom (whatever the person's own subjective view of the matter) and that the subtraction of options that are not objectively worthwhile does not diminish the person's freedom, whatever the person wants or thinks. My own view is that gaining the freedom to be a heroin addict represents an increase in one's freedom whether the life of a heroin junkie is objectively valuable or not. Also, I doubt whether there is an objective standard for determining whether heroin use is valuable for a person independently of consulting that very person's evaluative standpoint. But my doubts on these points do not shake my confidence that anyone interested in the topics of freedom, equality, or justice would profit from a close reading of this book.

*University of California, San Diego* RICHARD J. ARNESON

**Risk and Rationality: Philosophical Foundations for Populist Reforms.** By K. S. Shrader-Frechette. Berkeley: University of California Press, 1991. 312p. $39.95 cloth, $15.95 paper.

This book aims to provide a philosophically respectable populist position to pit against two allegedly antipopulist theories of risk. "Populist" means taking public complaints seriously and giving credit to the people's fears about rising morbidity and death rates due to technology. "Antipopulist" means dismissing these fears and giving reasons for supporting a coalition of government and industry against control of pollution and workplace dangers. "Antipopulist", in this book, includes two approaches to risk: the naive positivism of engineers and scientists, for whom a fact is a fact and has nothing to do with values and cultural theory, espoused by a handful of academics. If ever cultural theory needed proof that egalitarian sectarians create conflict to sustain their position, here it is. The two enemies may have been invented to add dramatic interest: 20 years ago, the naive positivists might have been hostile to the general thesis; but they are hardly threatening now, and the cultural theorists are nice people. Fortunately, being myself, a cultural theorist, I want peace, chiefly because the main argument is congenial to cultural theory.

Fighting artificial enemies on either side, Shrader-Frechette stands heroically in defense of the people and in defense of their risk aversion, too lightly dismissed by politically biased antipopulists. Risk theory does, indeed, tend to be muddied by political debating strategies; but that is the nature of the subject. Risk is inevitably politicized, because major values are at stake: employment, poverty, lives and regions laid waste, budgets emptied, timetables overturned. Disbelief as to the reality of dangers and slurs on the rationality of opponents are part of the rhetoric. Professional risk analysts try to evade the political aspects of their science and make claim to scientific objectivity. The author of this volume rightly pours scorn on such vain pretensions; but the argument falls into the other vanity and gets embroiled in philosophy of knowledge, rationality, and reality. Only at the end of the volume do important proposals for improving current methods of risk assessment appear.

One ray of light in the whole murky scene is that these proposals to confront political and ethical bias need to be seriously discussed. The idea is that risk-cost-benefit analysis could be improved by ethical weighting techniques, by requiring alternative analysis to take account of different ethical assumptions, and by weighting "expert opinions" according to their past performance. The defender of the people wishes to end arbitrariness in risk decisions and hopes that improved analysis will curb back-scratching, payoffs, bribes, and ignorance. Anyone familiar with environmentalist attacks on risk-cost-benefit analysis will recognize the courage of proposing to improve it by incorporating ethical and political biases, instead of discarding it altogether. The author implies that the result will be to depoliticize the issues and to give an opening for populist views.

It is probably true that risk-cost-benefit analysis as practiced fails to take account of egalitarian values, social obligations, and rights of individuals; ethical weighting will certainly open the field to a new kind of democratic debate. Passionate interest in carcinogens in parts to the million will be shifted to passionate interest in complex principles of ethical weighting. This would probably restore the prestige of science; for it is widely remarked that science, when used to arbitrate in politics, loses its authority. If the political strains are transferred to the scrutiny of ethical weighting systems, environmentalists may discover that the populist view is not necessarily in support of what seems to be obviously good and right. Eventually, the focus will be directed to cultural differences and to political disagreement. This is where cultural theory comes in, for it starts by mapping the political and ethical differences engaged beneath the surface of any argument about risks.

Cultural theory is devised precisely to understand deep-rooted political disagreement. Shrader-Frechette thinks that to acknowledge cultural difference is dangerously relativistic. Douglas and Wildavsky's *Risk and Culture* (1982) is singled for attack by misquotation, false note references, and blatant self-contradictions. The enemy theory is never expounded. Here it should be said that Douglas and Wildavsky are not relativists; they never say that any risk judgment is as good as any other or that all risk evaluations are equally plausible, nor do they ever accuse others of superstition or irrationality. Though this book conflates the lay public with the environmental movement, *Risk and Culture* expects public opinion to be more diversified and expects differences in cultural bias to be reflections of differences in other choices made by individuals. If the gross misrepresentation of cultural theory is typical, there will be other topics on which this book is misleading.

However, when the day comes for developing procedural rules for mapping risk dilemmas onto alternative ethical and political programs, cultural theory will be there to help. Eventually a cultural audit will take soundings of ethical and political bias so that scholars, politicians, and lobbyists who invoke populism will have better information. In short, cultural theory is a potentially powerful friend of the democratic project, not an enemy.

*London* MARY DOUGLAS

**Hegel's Political Philosophy: Interpreting the Practice of Legal Punishment.** By Mark Tunick. Princeton: Princeton University Press, 1992. 191p. $35.00.

This book is both more and less than the title promises. The title and parts of the author's introduction suggest a study of Hegel's ideas on legal punishment, a rather limiting theme because the issue of crime and punishment is an interesting but also a relatively minor part of Hegel's political philosophy. Tunick explores what Hegel has to say on this topic principally in his chapters 2 and 5. The larger component of this work is an account of the general framework of Hegel's philosophy of right through an examination of his concept of freedom and his idea of right as ethical substance. Throughout the study Tunick makes good use of student notes taken at Hegel's lectures on the philosophy of right. The lecture notes in question are unusually thorough and were until recently relatively unknown. They were used for the additions printed in Knox's translation of the *Grundlinien der Philosophie des Rechts* into English, but a complete transcription of the notes was only published in 1973 and 1974. They were augmented in 1983 by another set of notes from the lecture course of 1819/20 unexpectedly discovered by Dieter Henrich in the library of Indiana University. The lecture notes capture many of the comments and explanations made by Hegel to explain the otherwise very austere text of the *Philosophy of Right*. They must be used with some caution, as they are student notes after all, but in some instances they do much to illuminate even the fundamental points of this difficult book.

Regarding the issue of legal punishment, Tunick develops a lengthy argument to show that Hegel can be mined for an "immanent" criticism of social practices, including those of court and law, and that we today can learn something useful from him about how to understand the various issues connected with crime and punishment. On the basis of the argument made in this book, this claim does not seem very persuasive. Hegel, to be sure, offered an account of punishment as retribution and proposed the idea that punishment in some sense corresponds to an expression of the criminal's own will. These ideas are well worth pondering in themselves and in the context of his overall thesis about right. But does he truly step "inside" the practice of punishment in order "to resolve problems commonly faced by those working within the criminal justice system" (p. 108)? Tunick can show that Hegel has something to say on some of the issues confronting criminal justice practices in his own day. But there is insufficient evidence that he sought the close, even if dialectical, engagement with the details of practice that Tunick claims, and it is hard to believe that the "practitioners" (p. 108) whom Tunick has in mind would find Hegel's remarks very useful. Is it a surprise to learn that "Hegel says nothing about plea bargaining" (p. 124)? Most readers of Hegel understand that he is primarily "occupied with the profound and timeless, not the mundane and ordinary," as Tunick quite correctly concedes at one point (p. 108).

Tunick's approach to Hegel's thought is guided by a distinctly non-Hegelian thesis to which he returns frequently throughout the book. On the one hand, he presents Hegel as an important authority whom we ought to consult in order to illuminate "social practices" of his own time and of ours, but on the other hand he repeatedly avows a "sensibility" that is strongly at odds with Hegel's. Tunick rejects the "metaphysical" and "foundationalist" ideas that determine many of Hegel's most important concerns: "those of us who reject the idea that a practice can have a true meaning can reject Hegel's foundationalism . . . yet still draw upon his interpertations of the purposes of and principles underlying our practices" (p. 14). Hegel's view is "unattractive precisely for its dogmatism, for is insistence that we can be right or wrong about which commitments we should have" (p. 67). We need, he asserts, a "revisionist account" of Hegel, "for it lets us take advantage of Hegel's insights without compromising what I take to be a modern commitment to nonfoundational justifications" (p. 103). Tunick historicizes what is metaphysical or teleological in Hegel into social criticism freed of truth claims, and he prefers a Hegelianized Marx or an "existentialist" Hegel to Hegel himself (chapter 6). This point of view is more asserted than argued, and it is not, of course, original. Surely more original is the sexually-oriented language that Tunick employs in explaining how he applies this sensibility to the discussion of Hegel's relevance to the problems of the modern post-foundationalist world. Rather blatant comment about a woman unsatisfied by her marriage (p. 142) and a gratuitous sexual metaphor used in comparing Hegel and Marx (p. 158) will strike readers as quite unusual in the context of literature on Hegel's political philosophy. These comments make one wonder not only about the author's sensibility but also about what the editors at Princeton University Press had in mind when reviewing these pages.

*University of Oklahoma* DONALD J. MALETZ

# AMERICAN POLITICS

**Limiting Legislative Terms.** Edited by Gerald Benjamin and Michael J. Malbin. Washington: Congressional Quarterly, 1992. 324p. $18.95 paper.

**Restoration: Congress, Term Limits, and the Recovery of Deliberative Democracy.** By George F. Will. New York: Free Press, 1992. 260p. $19.95.

George Will is at his best when he is expressing outrage over silly governmental programs and when he is placing modern events in the context of long-standing philosophical strains of thought. Compared to Will, I dare say, no one is as adept at laying bare the lunacy of subsidizing mohair production, moving the Central Intelligence Agency to Robert Byrd's West Virginia, or limiting Jamaica to selling just 970 gallons of ice cream in the United States each year. And compared to Will, I dare say, no one is as capable of moving from Bob Traxler to Heraclitus in the space of a few sentences without sounding absurd.

Will regularly displays these two strengths in *Restoration*, a stimulating, spirited, and erudite attempt to convince readers that limiting the number of terms members of Congress are eligible to serve will improve the federal government. The trouble is that Will's typically masterful accounting of government gone amok and the obvious sincerity of his belief that everything would be fine again if we could just get back into the rhythm of the Founders' mantra make the main weakness of the book all the more apparent: never are we presented with anything remotely resembling a coherent description of the precise mechanism by which term limits will improve the polity.

The pattern followed throughout much of *Restoration* is that Will seizes upon something government is doing wrong and ravages the action or actions involved. Then, just when he should explicate how term limits would have kept all this from happening, we are, instead, presented with nonarguments like "Surely we would not be having this argument [on moving the CIA] if we had reasonable limits on terms" (p. 29) or "Does anyone doubt that the behavior of the Judiciary Committee [during the Thomas–Hill hearings] would have been markedly different—never mind better—if its members had been serving under a regime of term limitations?" (p. 112). Yes, someone does. And I am probably not the only reader of *Restoration* who thinks that even with term limits, we would still be debating the improper distribution of federal pork and that even with term limits, difficult personnel matters such as those involving the conflicting claims of Hill and Judge Thomas would probably still have been mishandled.

I cannot for the life of me grasp why term-limited legislators should be expected to demur from playing the pork-barrel-politics game. Senior members may be a little more adroit at the game (see Payne's *Culture of Spending* [1991], and this bothers Will; but senior members are also more adroit at behaviors Will applauds. Junior members, to take just one example, are more likely to introduce frivolous bills on an incredible range of unrelated topics, while senior members have more focused and realistic legislative agendas (see Hibbing's *Congressional Careers* [1991]). So when Will rails against the Washington mentality that every good idea out there should become a law, he is unwittingly making a case against term limits. The very members he would banish from Congress are in fact the ones most likely to exhibit some restraint in what they throw in the hopper.

To the extent that a rationale for term limits *is* provided in the book, it is that limits will alter the incentive structure of legislators. Because they know departure is unavoidable, legislators will decide to *do* good, rather than to *look* good. Limited terms, it is argued, will embolden members to tell constituents that mindless pursuit of pork would be bad for the country in the long run even though it might be beneficial to the district in the short run. As such, term limits would give people in the government "more constitutional space in which to think, more social distance to facilitate deliberation about the future" (p. 231). This is an interesting argument and is most atypical of term-limiters, many of whom see limiting terms as a way of bringing an out-of-touch legislative branch back into touch. George Will, on the other hand, believes that term limits will move the legislative branch just a little more out of touch, thus freeing it from the incessant pleadings of organized interests and the unrealistic demands (less taxes, more benefits) of constituents.

But term-limit proposals, particularly as currently constituted, would *not* generate a step-level shift in the incentive structure. Most of the states to date adopting term-limit language actually provide for rotation in office. This means that our ambitious but term-limited legislator has several options, including running for another political office, being appointed to another office, becoming a lobbyist, or running again for the recently vacated office after (usually) just two years. Now, the resulting game of musical offices may not be all bad but it would hardly produce fundamental alterations in the incentives of members to please the various interests hovering around Capitol Hill. Noble but politically suicidal actions would still be rare, pork would still be ladled, and scandals would continue to scar the Congress. In fact, the understandable desire to "leave options open" may actually make the very situation Will decries worse.

George Will's book usefully illustrates the diverse reasons people support term limits. Repeatedly, he distinguishes himself from the "slash-and-burn" conservatives, who wish to weaken government; the Republican party faithful, who think limits are the key to breaking the Democratic hammerlock on Congress; and Congress bashers, who simply want to—well—bash Congress. Point taken, if Will accepts a counterpoint, namely, that he has failed to recognize sufficiently the diversity of the other side. Not all of us who oppose term limits think Congress is perfect; not all of us have the same reasons for being skeptical of this particular reform; not all of us are knee-jerk defenders of professionalized politics. (A particularly withering passage late in the book makes a number of blanket statements about why political scientists are so strongly supportive of political professionalization.)

It is possible to oppose governmental subsidies for mohair production and also to oppose term limits. George Will should know this, for he did so in a previous incarnation. A variety of solutions exist for the problems Will identifies, and many of these alternatives are much more direct than the circuitous and occasionally convoluted reasoning necessary to connect term limits with the identified problems. Will dislikes franked mail. Would it not be simpler to ban franked mail than to try to address the problem by limiting terms? Will dislikes pork-barrel politics. Let us depoliticize the location of project construction just as we have depoliticized the location of project deconstruction. (I refer to the bipartisan commission that is central to decisions on which military bases to cut.) Are too many government-paid staffers involved in reelection-oriented activities? Cut personal staffs. Are too few challengers funded at levels similar to incumbents? Institute real campaign finance reform. Are representatives too sensitive to the people? Lengthen terms.

Toward the end of the book, Will uses a baseball analogy to illustrate that a team of eager, poorly paid rookies can sometimes outperform teams of complacent veterans. Permit me to counter with a different baseball analogy. If one team (called the Incumbents) is allowed to use short outfield fences, oversized cork-filled bats, and brightly painted superballs, while the other (called the Challengers) must use regular bats, balls, and fences, the Incumbents are likely to win 95% of the time, and the fans are likely to grow restive for real competition. Would it make sense to reject rules changes requiring both teams to play with the same bats, balls, and fences in favor of changes that would endorse the continuation of these inequities for eight seasons but then require the Incumbents to become agents, to play at a different stadium, or to wait two seasons before returning to the same stadium, eventually to the same unfair advantages as previously enjoyed? George Will's book would be stronger if he presented an accounting of other reform proposals followed by a ringing statement of why term limits are preferable to these other possibilities. Campaign finance, for example, is given nary a mention.

George Will's book might also be stronger if *Limiting Legislative Terms*, a collection of term-limit information and contributed chapters edited by Gerald Benjamin and Michael Malbin, had been available to him before he completed *Restoration*. Though they share the same topic, the books could hardly be more different—Will writing with an advocate's verve, Benjamin and Malbin striving to present a volume that is, on balance, agnostic even as some individual contributors hash out with appropriate abandon the pros and cons of limiting terms.

The volume begins with two strong essays on the history of the concept of limiting legislative terms: one (by Mark Petracca) going back to ancient Greece and one (by the editors) reproducing debate from the New York state ratifying convention of 1788. The longer middle portion of the book is not organizationally tight but is centered around informative essays on three of the four early states to vote on term limits (David J. Olson on Washington state, Charles M. Price on California, and Gary W. Copeland on Oklahoma). Also found here are chapters on limiting the terms of state executive branch officials (Thad Beyle) and the interest groups that have been active on term limits (Stuart Rothenberg), as well as shorter comments (Linda Fowler, Alan Rosenthal) and two essays (David H. Everson, the editors) taking up the risky task of explaining what legislatures under term limits will look like.

To this point in the book, all the authors, perhaps with the exception of Fowler (who minces few words in her opposition of term limits) have attempted to be reasonably neutral. But in the concluding section, the editors allow an assortment of contributors to vent their biases, sometimes with regard to the substantive benefits/detriments of terms limits and sometimes with regard to the constitutionality of state efforts to restrict citizen eligibility for Congress. These lively exchanges, plus some useful tabular material conclude this multifaceted volume.

The timeliness of the collection is both a blessing and a curse. Though the volume continues to be useful, it somehow seemed much more relevant before the 1992 elections (the publishers and editors were thoughtful enough to make prepublication copies available to many observers), when there was a more compelling reason to focus on Washington, Colorado, Oklahoma, and California. By now, 13 additional states have experienced term-limit elections and second verses have been played in Washington and California, so that much of the tabular and textual material already needs updating. But please note that many of the essays (even those targeted to certain states) make broader contributions to such topics as citizen politics, representation, reform, and political professionalization.

Scattered throughout this important debate, both in Will's book and in Benjamin and Malbin's edited volume, are references to the intentions of the Founders. Were they for or against term limits? I usually try to avoid this game because it implies that if we could discern the Holy Grail of unified Founders' preferences, we should follow it. Too much may have happened in more than two centuries to make this a sensible strategy.

I would much prefer to see a debate on the monumental issue that these works occasionally raise (and then quickly drop), namely, the complexity of governing as we approach the twenty-first century and the capability of ordinary citizens to comprehend and to provide guidance on the issues. Will thinks that governing is still well within the orbit of ordinary citizens, and I both hope and think he is right. But the reason he gives for this belief is that if this were not the case, representative government would be rendered difficult. Well, since our government would seem to be encountering persistent difficulties, perhaps issue complexity is part of the problem. This complexity *has* to put stress on the representational process as the intricacies of some issues outstrip the attention span of casually interested citizens. Rather than asserting away this problem, we need to study it, work with it, and mold government to it. What exactly this molding means, I do no know; but in spite of the well-intentioned efforts of George Will and others, I am yet to be convinced it involves limiting legislative terms.

*University of Nebraska, Lincoln* JOHN R. HIBBING

**Flying Blind: The Politics of the U.S. Strategic Bomber Program.** By Michael E. Brown. Ithaca: Cornell University Press, 1992. 358p. $47.50.

**Buildup: The Politics of Defense in the Reagan Era.** By Daniel Wirls. Ithaca: Cornell University Press, 1992. 247p. $31.50.

Defense and foreign policy making are much slighted topics in political science. Americanists seem driven to study almost every aspect of American politics but defense and foreign policy. For their part, defense and foreign policy analysts show much greater interest in expounding on what policy should be, rather than explaining why we get the policies we do. It is just this gap that Daniel Wirls and Michael Brown seek to fill.

The title of *Buildup* is somewhat misleading. The book examines not only the largest peacetime military buildup in U.S. history but also the domestic political events that eventually undercut the Reagan revolution in defense spending. In tracing how the United States went from buildup to "builddown" is eight short years, Wirls focuses on the political competition among the three leading defense coalitions of the 1980s: the Reagan administration, the peace movement, and the military reform movement. The analysis draws primarily on government documents and the secondary literature, supplemented with a smattering of interviews with government officials and political activists.

*Buildup* essentially proceeds in two parts. The first part begins by reviewing the relationship between military policy and domestic politics in the Truman-through-Carter presidencies. It then proceeds to a succession of chapters describing the origins, political organization, and policy goals of each defense coalition. The second half of the book is a series of case studies of the major policy initiatives of each coalition: the nuclear freeze for the peace movement, the Strategic Defense Initiative for the Reagan administration, and procurement reform for the military reformers.

What *Buildup* does best is tell the story of the politics of defense policy in the 1980s. Wirls does a solid job of exploring the origins of the three major defense coalitions and an equally fine job presenting the case studies. Perhaps the freshest material is his discussion of the military reform movement. As Wirls shows, the popularity of procurement reform in the mid-1980s owed in good part to the fact that many members of congress had found themselves squeezed politically in the clash between the Reagan administration and the peace movement. Procurement reform had the virtue of offering members a defense issue that was electorally appealing, regardless of their partisan affiliation. After all, "Who can be for waste, fraud, and mismanagement?" (p. 197).

*Buildup* disappoints in its failure to offer any general conclusions about defense policymaking in the United States. Although interesting insights pop up from time to time in case study chapters (and the discussion of military reform is best in this regard), Wirls makes no attempt to bring these thoughts together in a broader argument about how defense policy is made in the United States. What we are left with in the end is a great deal of very good description but very little guidance about what it means.

If *Buildup* has a misleading title, *Flying Blind* has a misleading subtitle. Anyone who picks up *Flying Blind* expecting to read an in-depth discussion of Jimmy Carter's bid to kill the B-1 bomber program or of congressional efforts to halt the B-2 bomber will be disappointed. Presidents and members of congress seldom figure in the story Brown tells.

In ignoring the broader political arena that is the central concern of *Buildup*, *Flying Blind* focuses, instead, on the organizational forces within the Air Force that have shaped the development of the U.S. strategic bomber program over the past 50 years. In doing so, Brown addresses two question: Why do bomber programs begin? and Why do some programs meet their cost, schedule, and performance targets, while most do not?

To answer these questions, *Flying Blind* reviews every strategic bomber program from the B-35 of World War II through the B-2 of today. The case studies go well beyond the standard secondary literature and draw on an imposing array of primary sources, including declassified Air Force documents (many of which were declassified at Brown's request), aerospace industry documents, interviews with Air Force and corporate officials, and congressional hearings and reports.

Brown draws two conclusions from the case material. The first is that strategic and, to a much lesser extent, bureaucratic influences explain the origins of weapons programs. Brown finds the economic explanations of weapons development offered Mary Kaldor, James Kurth, and others to be wanting. "The air force contacted the contractors, not the other way around" (p. 308). Brown also finds the technological explanations offered by scholars such as Morton Peck, Frederic Scherer, Solly Zuckerman, and John Steinbruner, and Barry Carter to be equally suspect. The evidence shows that, rather than being driven by technological opportunism, "the air force set its performance requirements far beyond the state of the art; they could not be met unless unforeseen developments took place" (p. 307).

The second conclusion Brown reaches is that whether a weapons program meets its cost, schedule, and performance goals depends largely on "the developmental objectives and procurement strategy on which it is based" (p. 305). As a general rule, the more technologically ambitious a program is, the better it is served by sequential, rather than concurrent, procurement. In practice, however, strategic and bureaucratic factors frequently lead the U.S. military to attempt to combine extremely ambitious development objectives with highly concurrent procurement strategies. The result is that developmental problems bedevil most weapons programs.

The strength of *Flying Blind* is the great wealth of material it presents. Without a doubt, *Flying Blind* will become a standard reference for scholars interested in the organizational history of the U.S. strategic bomber program.

*Flying Blind* well sustains the argument that strategic factors outweigh bureaucratic, technological, and economic factors in the initiation of weapons programs. One problem is that the book never establishes a standard for falsifying the strategic explanation. The military's need to convince Congress and the president to develop a bomber means that every program comes with a mountain of documents trumpeting its contribution to the national interest. What would the history of a weapons program have to look like to enable us to conclude that strategy does not matter? Answering this question is made all the more difficult by the fact that bureaucratic interests frequently define what constitutes a strategic threat. For example, even though the Soviet Union faced the same general strategic threat (nuclear annihilation) as the United States for four decades, it made very different choices about the size and composition of its bomber force.

The second problem with the conclusion that *Flying Blind* draws about weapons development is that it is, in all likelihood, time-bound. As Brown recognizes in the concluding chapter, most U.S. strategic bombers were developed during or immediately following World War II, precisely when one would expect strategic influences to be greatest. It is the programs initiated more than a decade after the war (the B-70, the B-1, and the B-2) whose development Brown find to have been greatly shaped by bureaucratic factors. This suggests that the claim that strategic factors are preeminent in weapons development may not hold true for the 1990s, a decade in which strategic threats are diminishing, while large, entrenched bureaucracies persist.

Despite their shortcomings, both *Buildup* and *Flying Blind* make solid contributions to the literature. I hope that their publication will encourage other scholars to explore defense and foreign policy making in the United States.

*University of Iowa* JAMES M. LINDSAY

**The Effective Republic: Administration and Constitution in the Thought of Alexander Hamilton.** By Harvey Flaumenhaft. Durham: Duke University Press, 1992. 314p. $32.50.

Harvey Flaumenhaft's thoughtful and meticulous study of Alexander Hamilton's political thought demonstrates more than any other work I know, with the exception of Harry Jaffa's *Crisis of the House Divided* and martin Diamond's essays on the American founding, that American political thought is essentially embedded in the writings and speeches of our leading statesmen, which would include, of course, our leading jurists.

Flaumenhaft goes as far as one can in extrapolating Hamilton's political principles from his writings and speeches without confusing his arguments with philosophical disputation. Thus, he preserves the distinction between political thought and political philosophy. Hamilton "was not a theoretical writer but a statesman." We hardly need to be reminded that America has produced no political philosophers, hence no political philosophy; but that does not suggest the absence of principles, nor would it in any way detract from the intrinsic worth of American political thought.

But it should be pointed out that this study is primarily a statement of Hamilton's principles—principles extrapolated from his statesmanship. The circumstances from which his arguments and recommendations emerged are not so apparent. As Flaumenhaft states:

> It is the aim of this book to set forth those political principles constitutive of Hamilton's thought. Hamilton's principles . . . lie scattered through the many volumes of official reports, newspaper essays, speeches, letters, and miscellaneous papers. Hamilton's collected writings require selection and arrangement to yield the principles that guided his handling of particulars.

Flaumenhaft correctly indicates that "histories alone cannot supply an adequate view of Hamilton's political science," but I am sure that he would agree that a political history of the great controversies of the 1790s between the Hamiltonians and Jeffersonians would make an extremely useful supplement to his volume. We would thus be able to secure a deeper understanding of the sense in which the resolution of these controversies contributed to the first phase of the completion of the American founding.

It becomes clear from reading Flaumenhaft's account that the tension between the Hamiltonians and Jeffersonians need not be considered debilitating (in spite of the fact that Washington understandably believed it to be so at the time); for constitutions worked out on paper, however solid they might be fashioned, need to be worked out in practice. Flaumenhaft indicates that Hamilton's political thought was concerned with completing the arrangements made in the Constitutional Convention in order to ensure a stable republican government and that that was precisely what occasioned the Jeffersonian—Madisonian opposition.

Flaumenhaft successfully disposes of the charge generated by the Jeffersonians that Hamilton was a monarchist, that he sought to erect a monarchy on the ruins of republican government (something that I think that neither Jefferson nor Madison really believed), while at the same time showing how Hamilton attempted to blend the advantages of a monarchy and republic in the American Constitution. Hamilton wanted to infuse republicanism with excellences learned from monarchy through the subsumption of an independent energetic executive, what might appropriately be called limited energetic government. Hamilton's criticism of the Articles of Confederation as early at 1779–80 was that the legislature tried to play the executive and that a reform of the government would involve the creation of an independent executive through separation of power.

The concluding chapter restates Flaumenhaft's arrangement of Hamilton's arguments articulated in the previous chapters of this work, revealing to the reader the remarkable consistency of his political thought. He concludes by suggesting that we can deepen our understanding of republic principles from an examination of Hamilton's theory and practice but cautions, "It is not my aim to facilitate a judgment on the correctness of Hamilton's judgment about the *particulars* of the political controversies and policy decisions in which he took so large a part." However, presenting the argument as carefully as he does, he inevitably facilitates a judgment as to its correctness.

Flaumenhaft, in explaining his method of presentation, indicates that he uses "the words of Hamilton himself as much as possible often omitting quotation marks—conflating, compressing, and rearranging Hamilton's phrases and sentences without constantly repeating 'Hamilton says'." Flaumenhaft and Hamilton become almost the same person so that we hardly know where one stops and the other begins. That being the case and in the absence of any serious criticism of Hamilton, we must assume that Flaumenhaft is in substantial agreement with him on principles if not on practice. Unlike Jaffa, who, overtly pro-Lincoln in his *Crisis of the House Divided,* makes the case for Douglas first, Flaumenhaft never makes the case for Jefferson or Madison, Hamilton's foremost opponents.

*Northern Illinois University* MORTON J. FRISCH

**Weapons of Influence: The Legislative Veto, American Foreign Policy, and the Irony of Reform.** By Martha Liebler Gibson. Boulder: Westview, 1992. 188p. $38.50.

Martha Liebler Gibson's book assess the impact of the loss of the legislative veto, declared unconstitutional in *Immigration and Naturalization Service* v. *Chadha* in 1983, on congressional–executive relations in the area of foreign policy. Most observers have argued that the loss of the veto has greater implications for domestic than for foreign policy. Gibson contends that that conclusion is based on a failure to recognize variations in the type of foreign policy to which the veto applied and an overreliance on the case of the War Powers Resolution. Rather than reduced congressional involvement in foreign affairs, her examination reveals that legislative-executive conflict has become more acrimonious and public as Congress seeks to adapt to the limitations imposed on it by *Chadha*. The problems is that the Court, in striking down the veto, offered a legal solution to a political problem.

Gibson presents a theoretical framework for understanding the effects of the loss of the veto in the foreign policy arena. Her dependent variable is the intensity of conflict between the branches. Such intensity will be affected by the institutional motivations of congress, electoral motives, presidential claims of authority over a policy area, and the congressional will to act. In the absence of either institutional or electoral motivations (or should they conflict), Congress is unlikely to muster the collective will necessary to oppose the president. Should those motivations be present, Congress is more likely to pick a fight. Gibson's theory is that the independent variables necessary to escalate conflict vary systematically by the type of foreign policy at issue. Neither symbolic–ceremonial nor crisis decisions are apt to lead to conflict. In contrast, the "intermestic" (international–domestic) and strategic (affecting international balance of power) areas invite conflict. Strategic policy is further broken down into salient and nonsalient areas, conflict being more intense in the former.

This typology is illustrated by a series of case studies documenting the intensity of congressional–executive conflict pre- and post-*Chadha*, from the 95th through the 101st Congress. Quantitative measures (e.g., the number of bills and resolutions and disapproval introduced, the number of hearings held on a topic) are presented, along with qualitative assessments. In the area of arms sales to the Middle East, an example of strategic–salient policy, conflict was both intense and visible. This was true, as well, for the case of most-favored-nation trade status for the Soviet Union and China. In both of these cases, the real catalyst for congressional action seems to have been electoral pressure. In the cases of arms sales and trade status of the Soviet Union, the powerful pro-Israel lobby energized Congress (owing to the immigration status of Soviet Jews), while public opinion in general turned against China following the events in Tienanmen Square in 1989. Ephemeral public opinion reduced the electoral motivation for Congress to act in the area of nuclear nonproliferation, the example of strategic nonsalient policy. And in the case of crisis policy, Gibson contends that Congress was never motivated to invoke the veto in the War Powers Resolution; thus, its loss did little to alter the relations between branches in this area of foreign policy.

While this theoretical framework is plausible, the case studies that illustrate it constitute an insufficient test of the thesis that the loss of the legislative veto will inevitably lead to conflict in some types of foreign policy but not in others. The test is insufficient because in each policy area, too few examples exist. In the case of arms sales, it is only arms sales to the Middle East that led to controversy. Arms sales to other regions failed to provoke conflict because those regions were insufficiently salient to any important constituency of Congress. But if only sales to the Middle East can be identified as strategic–salient one, wonders how general a category this is. Perhaps, as others have contended, arms sales to the Middle East are a unique case. Similarly, one wonders if the case of most-favored-nation status for China is a similar outlier. Had Tienanmen Square never taken place, it seems most likely that presidential preferences would have prevailed in this case, with little comment from Congress. The problem is that the typology seems to be developed from these cases; therefore, they cannot be used to test it.

A second threat to the adequacy of the contention that the loss of the veto led to heightened conflict is the failure to acknowledge the issue of divided government explicitly. While the time frame for the study includes periods of divided government before and after the veto was struck down, the fact that Ronald Reagan's honeymoon with Congress (or at least the House) was over after the 1982 elections should be considered.

The theory can be simplified to say that the loss of the veto matters when constituency pressure give Congress both the electoral motivation and the will to counter the president. While this may be more likely in some types of foreign policy than others, it could theoretically occur even situations that began as crises. It would be more parsimonious to argue that when any type of foreign policy has—or develops—a constituency that will push Congress to oppose the president, conflict will escalate.

Whether the theory is right matters, given the antidote for acrimony that is suggested. Gibson argues for legislation requiring joint resolutions of approval in areas where conflict is likely to occur. But unless conflict can be predicted with certainty Congress will need to include such resolutions in all foreign policy legislation. Another problem is the probability of this solution. Gibson's belief that such resolutions would force Congress to accept responsibility for decisions is probably correct. Given the congressional penchant for avoiding responsibility, such legislative action is unlikely.

As a description, *Weapons of Influence* is a good chronicle of Congressional–executive relations; but its principal theoretical argument requires validation through further testing. The Clinton presidency affords an opportunity to do so while ruling out divided government as a cause of high-profile conflict between the branches.

*Texas A&M University* PATRICIA A. HURLEY

**What Does the Lord Require? How American Christians Think about Economic Justice.** By Stephen Hart. New York: Oxford University Press, 1992. 253p. $24.95.

**The Transformation of the Christian Right.** By Matthew C. Moen. Tuscaloosa: University of Alabama Press, 1992. 224p. $26.95.

Two encouraging signs of scholarly sophistication and depth mark the developing study of religion and politics: the diversity of research and integration into the body of theoretical work in political science. Stephen Hart's *What Does the Lord Require?* represents the former, Matthew C. Moen's *Transformation of the Christian Right*, the latter.

Attitudinal research on the influence of religious beliefs and values on political behavior has most often come from the fields of voting behavior and survey research. Such research has broadly described some aspects of the relationship but necessarily lacks depth. Types of political behavior and political/policy attitudes can be relatively well defined; but the religious variables have, until recently, been unsophisticated (denomination, fundamentalism scales, and the like). Nor can such methods capture how ordinary people describe and interpret their own beliefs and behaviors, their rich inner lives.

Hart's book, based on two-to-three-hour individual interviews conducted in the late 1970s, explores how "ordinary" persons of diverse Christian commitments articulate the connection between their faith and their views of economic justice. Hart stands in and explicitly allies himself with such in-depth explorations of civic language as Bellah and colleagues' *Habits of the Heart* (1985) and Hochschild's *What's Fair?* (1981). Although he supplements the interview material with brief accounts of selected surveys, the latter are not used very effectively. The book's strength lies in its rich descriptions of how people do, in fact, make sense of their beliefs.

Moen, on the other hand, stands squarely in the tradition of political science elite research. His primary data come from interviews with congressional staff and lobbyists, whose worlds are quite different from the citizens interviewed by Hart. Moreover, Moen's subject is the most frequently researched religion-and-politics topic of the last decade-and-a-half, the Christian Right, placing it squarely within the interest group literature, a strength. Moen here provides the most complete description yet of the rise and transformation of the Christian Right at the national level from the late 1970s until 1990. There is no better, more accessible, or more balanced account available. This book follows well his earlier *Christian Right and Congress* (1989), which told the story up to about 1985.

Moen's argument and the structure of his research are straightforward. He contends that the Christian Right, through its experiences in Washington lobbying and electoral politics during the 1980s, has become both more politically sophisticated and more secular. Chronicling changes in organizations and leadership, he demonstrates that the movement learned from both its early mistakes (including tying its fortunes to the Reagan administration) and agenda-setting successes. This is the *sophistication* thesis. Although the Christian Right does not possess its former public visibility on Capitol Hill and White House access, the organizations that have survived are better structured for the long haul and have carved out specific issue niches where they have a chance to be effective. They have, in short, both learned how politics works and become more like other interest groups. Moen connects his interviews with media accounts of the Christian Right and with both general political science interest-group literature and other research on the Christian Right to make this argument.

The *secularization* thesis is a bit more difficult to describe, and Moen sometimes means two different things by it. His primary meaning (and the most important) is that the Christian Right has changed its morally charged and explicitly Christian rhetoric of the early 1980s to language more acceptable in a liberal pluralist society. Specifically, Christian Right groups have adopted the liberal language of rights and choice to stake their claims regarding school prayer, textbook selection, and other issues. Moreover, they have adopted the language of "victimization," claiming that Christians (or, at least, fundamentalist–evangelical Christians) have been unfairly marginalized by the media and by dominant political and cultural institutions. Moen's point is correct and well documented, and it supports his sophistication thesis. Political success demands that we speak the language of the dominant liberal (in the broad Lockean sense) political culture.

Moen is on shakier and less important ground when he cites evidence of hypocrisy on the part of some religious figures on the Right as evidence for secularization. That political expediency overwhelms principle, religious or otherwise, is a constant of politics, not a strategic movement.

The secularization theme provides a good connection with Hart's account of the language of ordinary Christians. Both liberalism and (particularly) Protestant Christianity contain healthy doses of voluntarism. Thus, it is relatively easy for both elite and ordinary Christians to move from a focus on the individual choice that must be made to follow Christ to the liberal language of choice and rights; that is, Hart shows better than Moen not only that there are strategic political reasons to become more "secular" but that both the religious and political language of America strongly encourage such a move.

This kind of argument is the strength of Hart's book. He is able to take central religious themes (especially voluntarism, love, universalism, this-worldliness, and otherworldliness) and describe how Christian citizens use such themes to describe their beliefs and justify their political stances. Based on these descriptions, he argues convincingly that Christian faith does not have unidimensional economic interpretations and implications. Hart also contends that attention to such language can enrich a public discourse overwhelmed by rigid ideological classifications (124, 214–215). His interviewees held impressively consistent views but ones that did not always follow conventional ideological lines.

Hart's book does have some limitations from the perspective of political science. First, it is rooted in sociological methods and theories. This is not in itself a problem. Political science can learn from long-established research in the sociology of religion. But Hart is not well versed in the recent behavioral research on religion and politics and does not integrate his findings with it. Second, though his methodological appendix acknowledges the generalizability problems of interviews conducted in 1976 largely in the San Francisco Bay Area, he minimizes these difficulties more than he should. It is not clear that citizens today would use the same languages of religion, politics, or economic justice that his respondents did 15 years ago.

Hart and Moen provide a nice contrast. Moen focuses on the relationship between religion and conservative politics. Hart, although he fairly considers how his subjects' religious beliefs can and do support a variety of economic ideologies, is more interested in the Left. He himself is a Christian Left activist; and his final chapter is devoted to exploring, quite judiciously, how the Left could use his findings. Taken together, these books demonstrate that the effect of religious belief on political action in the United States is up for grabs. There is no particular reason why Christians (the subjects of both works) should always identify with the Left or the Right. Not only Moen's self-consciously ideologically consistent activists but also Hart's less sophisticated citizens have thought deeply about how their fundamental religious principles affect their political perspectives; and they have arrived at different conclusions.

Neither of the books is ground-breaking, and neither is without flaws; but taken singly or together, they represent important deepening of our understanding of the relation between religion and politics. They suggest an open field of interaction during the 1990s.

*Texas Tech University* CLARKE E. COCHRAN

**The Politics and Economics of Privatization: The Case of Wastewater Treatment.** By John G. Heilman and Gerald W. Johnson. Tuscaloosa: University of Alabama Press, 1992. 235p. $24.95 paper.

In their book, *The Politics and Economics of Privatization*, John Heilman and Gerald W. Johnson have successfully staged an intellectual coup. They have been able to make theory (sophisticated, multifaceted theory, at that) applicable to the practice of government (decisions that persons with legal responsibilities have to make to meet public demands). The subtitle suggests that the book is a case study about wastewater treatment policymaking or possibly one particular wastewater treatment plant decision. Rather, this is a book abut theory and hypotheses, using a substantial number of well-researched wastewater treatment facility decisions in the 1980s as a testing base.

The authors have a specific definition of privatization that assists analysis. It is not the generic definition usually employed by economists to refer to any action that tends to marketize the operations of the public sector. The generic term they use is *public–private partnership* of which *privatization* is but one subtype. Privatization is defined in the wastewater treatment context as occurring when the privatizing firm "actively undertakes and coordinates all major phases of project development and implementation, including design, finance, construction, operations and maintenance, and ownership."

All too often, the subject of privatization is left to economists who assume that all situations would improve if the private-sector market mechanism were substituted for public-sector nonmarket ownership and operations. Political scientists, increasingly estranged from government and little conversant in public law, find themselves doing derivative, abstract research based on economic models and assumptions. Heilman and Johnson challenge this economic hegemony of political science. They truly understand public choice theory; but they also understand public law, administrative theory, and contemporary research methods. They recognize the profound truth that public–private partnerships, particularly the privatized variety, are extraordinarily complex social phenomena requiring sophisticated practitioners.

They wisely begin from the assumption that the primary purpose of government (beyond public safety, etc.) is the management of incentives (not the redistribution of wealth). In the 1980s, three tax laws were passed that radically altered the incentives for financing decisions on wastewater treatment facilities. The first law gave major incentives to financially strapped local governments and authorities to look toward the private sector for monies and talent. The other two laws drastically reduced these incentives. During the period of positive tax incentives for privatization, a variety of wastewater treatment projects were undertaken in a number of political jurisdictions. The authors reviewed some 25 privatization cases, applying both aggregate and project-specific data analysis, questionnaires, and interviews. They compared these cases with instances where the privatization option was rejected. They were therefore able to compare the political decision-making processes, financial arrangements, regulatory compliance requirements, and basic management approaches of the two systems.

To summarize their rich findings, suffice it to say that the decision to follow the privatization option necessarily means that the political jurisdiction becomes involved in an extraordinarily complex process with heavy emphasis on legal contract writing. The authors provide interesting insights into the impact of national policy (in this instance, tax policy) upon state and local governments. Once you view the principal role of government to be that of managing incentives (the theoretical underpinning of the study), the utility of the findings for political decision makers is greatly enhanced. The whole question of accountability begins to make sense. Economists rarely view political accountability as a critical variable in their privatization literature. To political scientists, however, political accountability is a critical factor to consider in public-sector decisions. It makes a difference, after all, whether the private wastewater treatment facility owner or the local government is going to be held accountable by the public for the wastewater treatment operation.

The authors reach the conclusion that

> Privatization is a feasible and viable option to meet, efficiently and productively, some infrastructure needs. However, in the public–private partnership movement, privatization, at least successful privatization, is a paradox. Rather than reducing government, which it may do in some ways, more fundamentally it brings the private sector into the policy and management structures of the public sector. The private sector becomes more like a public sector than the reverse. (p. 190)

Heilman and Johnson have produced a sophisticated, broad-ranging, theoretically-based study on the politics and economics of privatization (as promised by the title) without resorting to social science jargon or mind-numbing statistical matrixes. This book can be read with profit by scholars interested in methodology, as well as by government officials trying to develop institutions appropriate for the complex demands of the coming century.

*Congressional Research Service* RONALD C. MOE

**The Dynamics of Conflict Between Bureaucrats and Legislators.** By Cathy Marie Johnson. Armonk: M. E. Sharpe, 1992. 200p. $42.50.

Cathy Marie Johnson has written a book that explores conflict between congressional committees and federal agencies. She has a mix of conceptual, empirical, and normative concerns in which she sets her analysis of "the relationships between four bureaus and their respective authorizing committees from 1961 to 1984" (p. 4). The four bureaus are the Bureau of Reclamation, the Bureau of Indian Affairs, the Food and Drug Administration, and the Social and Rehabilitation Service.

The author wants to answer the questions when and why conflict occurs between these agencies and their authorizing committees. She succeeds in describing the frequent conflicts in these four cases. She is less successful in grappling with conceptual questions and matters of evidence.

Like many scholars who interpret relationships between actors engaged in policymaking, Johnson uses a mix of documents, relevant scholarly publications, *Congressional Quarterly Almanac*, the *National Journal*, and interviews to provide data. The four core chapters (one dealing with each agency) present good pictures of the major issues that caused controversy between each agency and the relevant congressional committees during the years under consideration.

Johnson lays out two verbal "models" of the American national policy process: the *self-interest* model (which also has a subgovernment variant) and the *policy* model. She finds the former inadequate and offers the latter as more helpful in understanding the dynamics of the relations between federal agencies and their authorizing committees. The discussion of the models points to some major differences in perspective on the part of different scholars. However, her discussion of the self-interest model oversimplifies important differences between various formulations of both the general model and its subgovernment variant. Lowi, Fiorina, Salisbury, and others have more disagreements with each other and more subtle analyses than her discussion would suggest.

A summary of her discussion of the competing models in tabular form would have been helpful, as her claims about the competing versions and the hypotheses derived therefrom would then have been clearer. As presented, it is probably more accurate to say that she discusses two broadly different *perspectives* (rather than models). The hypotheses derived from these perspectives are *explored*, not tested, in the discussion that follows. These more modest claims would, I think, be more in line with what is offered and would have made the book more convincing.

Johnson, in discussing her four cases, makes the good point that conflict arises frequently between authorizing committees and the agencies for which they are responsible. This is true even in relatively cohesive subgovernments dealing with distributive policy. It is also true in the regulatory and redistributive arenas. Unfortunately, she does not seek to present evidence on the relative frequency of conflict and cooperation in these different cases. She is persuasive in arguing that conflict exists in all situations, but the reader has no way of estimating the relative amounts of conflict and cooperation.

This may be one instance in which her dismissal of the utility of attempting to quantify some of these relationships is too sweeping and too limiting. Qualitative case studies and interpretations are, by definition, incomplete and frustrating. Some quantitative measures of the incidence of cooperation and conflict could surely have been devised and would have been helpful. I agree with her that an exclusively quantitative study of these relationships would not be desirable, but I think she goes too far the other way.

Her study might also have been enriched by including the budgetary dimension in her case studies. Relations between appropriations subcommittees and agencies are also very important. A merging of some of the concerns of such scholars as Wildavsky, Sharkansky, LeLoup, and Fenno with her own focus on questions of authorization would have provided a fuller picture of the relationships she is analyzing.

Despite problems in presenting a compelling general argument, Johnson does make a number of general statements that represent truths too often forgotten: that policy goals, not just self-interest more narrowly defined, matter to actors in the policy process; that actors inside various subgovernments do, in fact, disagree; that harmony and consensus are not unchanging; that committee–bureau relations are always worth investigating in any policy area; that conflict over any specific matter does not necessarily disrupt normal working relations in general; that agencies do not always want to expand; and that conflict has some positive values in the development of policy.

*Ohio State University* RANDALL B. RIPLEY

**The Myth of the Independent Voter.** By Bruce E. Keith, David B. Magleby, Candice J. Nelson, Elizabeth Orr, Mark C. Westlye, and Raymond E. Wolfinger. Berkeley: University of California Press, 1992. 224p. $35.00 cloth, $13.00 paper.

The title of this book tells much of its story. The all but universal agreement of scholars and journalists that the last decades have marked the emergence of the independent American voter is, the authors assert, simply wrong. What has emerged, instead, is the willingness of large numbers of basically partisan voters to style themselves Independent in response to survey questions about party identification. The growth in the ranks of self-declared Independents occurs almost wholly among the so-called "leaners"—those willing to say, after declaring independence of party, that they are, indeed, closer to one of the two political parties than the other.

The crucial story and much of the empirical case of the book is that these "leaners" are in thought and deed much like self-declared partisans, often more so than those who claim a partisan identity that is "not very strong." Noting that, as an empirical observation, this is not new, the authors take us to task, instead, for not appreciating its significance. If one classifies the leaners with the party to which they claim closeness, then the oft-noted dealignment simply does not happen. There is no appreciable growth, that is, in the ranks of the remaining "pure" independents. By extension, the party alignments of 1992 are those of 1952, changed, in the main, because many partisan voters now like the ring (but not the behavioral consequences) of "Independent."

The propensity to lean, rather than identify is a mystery to be solved in the book. The authors attack from several directions, with major analyses of generation, issues, and political alienation as possible explanations. They conclude, with notable scholarly modesty, that nothing really seems to provide any leverage. The reason that the distinction fails to yield to all these analyses is that it is superficial. The problem to be explained, they write, "is not a pattern of belief, behavior, or commitment. What is to be explained is a questionnaire response" (p. 200).

This book is vintage voting-behavior in style and focus. Its data come entirely from the National Election Study series. Though the authors are nimble in their exploitation, these data are also its limit. They tell us about the responses of survey respondents in election seasons and a bit about why those responses are what they are. But the "independent voter" is not only a matter of voting behavior—perhaps not even primarily so. The topic lies at the intersection of issues of voting, party systems, and political parties generally. It is about how politicians campaign and then govern, what elections mean, and more. The empirical basis in the book is voting alone. Thus, its suggestions that misunderstanding the "myth" has major implications for parties and party systems remain only suggestions. I wish the authors had ventured further away from the survey data to explore those implications.

The book leaves us with a quandary. If its major thesis is correct, then partisanship is as widespread now as ever. But to the degree that we have salvaged partisanship as a proportion of the American electorate, we have equally debilitated it as a causal force; for this now-widespread partisanship must be peculiarly weak if the effects alleged to result from dealignment (e.g., regular minority party victories) are real. If they are and if most voters are partisans, then partisans as a group must behave in much the manner presumed of independents in order to produce the electoral result we see.

*University of Minnesota* JAMES A. STIMSON

**Information and Legislative Organization.** By Keith Krehbiel. Ann Arbor: University of Michigan Press, 1991. 328p. $24.95 cloth, $14.95 paper.

Three decades ago Robert Dahl observed in *A Preface to Democratic Theory* that Madison's worry about tyranny of the majority should be turned on its head. In the United States, Dahl wrote, intense minorities, not majorities, typically control the policy choices that emerge from institutions that we typically label "majority rule." While seldom expressed in Dahl's terms, this view has become common in both academic and popular characterizations of the United States Congress. The weak party, strong committee nature of legislative organization enables members with intense interests in particular domains to control agendas and dominate outcomes. On any given matter, a relatively disinterested majority within Congress trades away its constitutional right to decide, while its members, in return, anticipate legislative advantages in domains of their own choosing. The self-selection of interested members onto committees, the allocation of agenda power and other parliamentary rights, the assignment of restrictive rules to bills in the House, and the procedural advantages awarded committees at the post-floor stage, in turn, restrain agents of nominal majorities from undoing, bill by bill, the cross-committee logrolling arrangements that redound to the electoral benefit of legislative incumbents. Policy benefits, then, are distributed to members whose districts value them most; thus structured, the legislative world is a good sight better for every member than the War of All Against All.

In *Information and Legislative Organization,* Keith Krehbiel launches a frontal assault on the conventional view. The distributive theory of legislative organization, he concedes, is plausible, even intuitive, and it squares with what he takes to be a large body of largely anecodotal evidence. But there is an alternative view, one which emerges from recent theoretical work on games of incomplete information that he and colleague Thomas Gilligan have elaborated in a series of papers over the last several years. The view that Gilligan and Krehbiel have advanced turns on a distinction that is central to the incomplete information framework they employ, between policies and outcomes. Policies are the objects of legislative choice, i.e., bills and their various provisions. Outcomes are the states of the world that policies are intended to affect and that members (and their constituents) ultimately care about. Members have well-defined preferences over outcomes but not policies, as members are uncertain about whether and to what extent proposed policy alternatives will have their intended effect. Information about the connections between desired outcomes and alternative policies is thus a valuable commodity, both for members individually and Congress collectively. The central problem of legislative organization, then, is to design institutional arrangements that lead legislators to develop policy expertise and to share that expertise with their otherwise ill-informed colleagues.

In elaborating and defending his theoretical challenge, Krehbiel has provided us with an immensely important book. It is innovative, empirically rich, and very well-written—a rare combination, to say the least. There is little doubt that the pork barrel is an overworked metaphor for understanding legislative politics. It certainly captures one important part of legislative politics, but, like all metaphors, it highlights certain features at the (implicit) expense of others. In building his approach to legislative organization up from informational foundations, Krehbiel brings important features of the institution into sharp relief. From Krehbiel's angle of vision, for instance, such things as (sub)committee hearings, committee reports, floor debates, and other public statements by interested actors become more than just sideshows to the *real* business of aggregating brute preferences. Such activities reveal outcome-relevant policy information, such that those with more relevant or more credible information are able to affect policy choices through genuine *persuasion,* not simply through bargaining, rewarding, sanctioning, or procedural maneuvering.

By itself, this is an important move for those who pledge allegiance to rational choice yet simultaneously claim that their goal is to understand real, rather than reified, legislatures. The assumption that policy preferences are fixed (not to mention that information is complete) has always troubled more inductive scholars, who have wondered more than once that the positive theory of institutions might never move beyond the business of developing elegant models of irrelevant worlds. Moreover, Krehbiel's approach takes legislative scholars an important step toward securing the premise of rationality itself (not that Krehbiel himself thought it in need of any help). Why would so many reasonable people waste so much of their (and their staff's) scarce time and energy in floor deliberations, committee meetings, and interactions with (or reading material from) committee staff, the Congressional Research Service, the GAO, interest group lobbyists, and the like? Either information and debate must matter to legislative decision making, or the rationality of legislators as resource-allocators must be foresworn for the view that their heads are filled with cornmush.

The hypotheses that follow from Krehbiel's informational perspective on legislative organization are, to say the least, distinctive. According to the distributive view, for instance, committees will be homogeneous and biased in their preferences, relative to the parent chamber. According to Krehbiel's informational account, committees should be heterogeneous and representative; if there is any favoritism, it is shown to members with relative expertise rather than strong distributive impulses. According to the distributive view, restrictive rules will be awarded to biased committees so as to protect the larger cooperative logroll from some majoritarian maverick with a mischievous floor amendment. According to Krehbiel, the chamber is *least* likely to assign restrictive rules to biased committees trafficking in distributive benefits. According to the distributive view, finally, the parent chamber provides standing committees with special post-floor parliamentary rights (i.e., control of the House-Senate conference and hence an ex post veto). Such prerogatives, in turn, insure that majoritarian mavericks will anticipate little ultimate gain from their floor mischief and, in any case, insure that the committees can probably undo in conference any serious damage that might have been done to their bill at some earlier stage. Krehbiel, on the other hand, hypothesizes that post-floor decision processes "should be characterized by selective and carefully monitored delegation of parliamentary rights to relatively expert members of standing committees" (p. 199).

In the first three chapters, Krehbiel elaborates this debate between distributive and informational theory in an impressively thorough and entirely accessible fashion, with nary a formal proof in sight, and for good reason. One purpose of the book is to bridge the gap between formal and empirical scholars, and this one does that as well as any I've read. But more to the point, this is first and foremost an *empirical* book, a submission of alternative hypotheses from competing theoretical camps to empirical tests. Hence, Krehbiel takes each of the competing claims listed above as the point of departure for chapter-long empirical investigations. These chapters are impressive in numerous respects. Krehbiel relates his arguments to both the formal and empirical literature; he uses both legislative and non-legislative examples in clever and informative ways; in each chapter, he employs an impressive mix of primary and secondary evidence, ranging across insider and outsider accounts, qualitative and quantitative material, and data from both chambers and different historical periods. What else could one ask? This is great reading.

Given the ambitious nature of Krehbiel's challenge, of course, it should surprise no one that Krehbiel's arguments and evidence are not without their limitations (nor does he claim otherwise). Conventional wisdoms do not linger in a community of skeptical scholars without more than a little supporting evidence and reasoning. In my view, several limitations bear mentioning here, in that they reflect on how the book should be read and where it should push the next generation of research on legislative institutions.

Perhaps the easiest criticism one can make of Krehbiel's book is that its principal arguments are overstated. While there are qualifications and disclaimers here and there, one cannot avoid the impression that Krehbiel is (rightly) pushing out one stylized, overworked view of legislative organization (distributive, decentralized) only to replace it with an underworked but still overly stylized view (informationally efficient, majoritarian). At times Krehbiel reveals this worry; for instance, at one point he (I think rightly) characterizes his approach as the informational "supplement" to distributive theory. But for most of the book he sets up the search for explanation as a stark contrast—a contest between two competing sets of categorical claims, with the winner to be decided in a trial by data (which belies a faith in data that even we at Michigan don't have).

This is, I think, a forgivable flaw, if indeed it is a flaw at all. Krehbiel is making an important challenge to a large body of theoretical and empirical work on legislative organization that has accumulated over several decades. It is probably useful at this point that he make the challenge in its clearest and strongest terms. Indeed, formal theory is at its creative best when it pushes us to entertain non-intuitive possibilities, which in turn cause us to reexamine our settled view of the world we are studying. This book does that.

A second and related limitation concerns the empirical evidence that Krehbiel marshals to support the informational perspective. On the one hand, one cannot help but be impressed by the breadth and eclecticism of his empirical efforts. Using illustrative examples, comparative cases, qualitative interpretations, and a great deal of quantitative analysis, Krehbiel makes a convincing case that the distributive view of legislative organization is, if not wrong, certainly incomplete and almost certainly exaggerated in its applicability to the U.S. Congress. For instance, his reinterpretation of Kenneth Shepsle's results in *The Giant Jigsaw Puzzle* (1978), one of the cornerstones of the distributive literature, is clever and mostly convincing; he shows that Shepsle's own analysis of the committee assignment process is consistent with the view that members sought and received assignment on the basis of their committee-relevant expertise as much as their desire for district-specific benefits. And Krehbiel's analysis of special rules and post-floor procedures is among the best to date, superior in the quality of data and the sophistication of the analysis to the recent work of more strictly empirical scholars.

Impressive in many respects, however, the empirical analysis is uneven in how much it convinces. I have argued elsewhere (with Bernard Grofman, this *Review*, December 1990) that Krehbiel's refutations of the committee bias hypothesis rest on inappropriate tests and data of questionable validity. Likewise, his conclusions regarding restrictive floor amendments may be due more to poor measurement of the distributive variables than the theoretical weakness of the distributive hypothesis. More generally, Krehbiel's quantitative analysis too often exhibits a tendency to misinterpret the meaning of statistically insignificant results regarding distributive claims when Krehbiel has his own competing hypotheses on the line.

My final concerns go not to matters of evidence but to a lingering theoretical puzzle that, as I read it, remains troubling to Krehbiel's informational account. If, as Krehbiel correctly argues, specialization creates an informational efficiency that produces a public good (more informed but still majoritarian collective choices), how is it that the assembly overcomes the Olsonian illogic of collective action. That is, why don't rational legislators simply free ride? If, as Krehbiel argues, committee specialists get little in the way of distributive benefits, why doesn't Representative Krehbiel sit back and let others do the labor-intensive specialization while he reaps the benefits of a more informed policy choice? To put the puzzle in somewhat different terms, why don't members *avoid* committee assignments (which is what I do) rather than seek them out (which is what they do)? Insofar as I can tell, Krehbiel does not adequately address this until a subsequent discussion of floor procedures, where he smuggles back in a bit of distributive theory. The committee specialist gets a little "greater payoff," a "distributional commission," or a "distributional bonus." This I take to be a persuasive move; indeed, it reinforces the point made above that in most of the book Krehbiel pushes too much the informational hard line. But at the same time, by entertaining some need for distributional commissions, Krehbiel moves onto a slippery slope. "Preference-outlying committees," Krehbiel concedes, "are distributionally nonmajoritarian if, to induce them to specialize, the parent chamber must commit to the use of a closed rule for the committee's proposal." Fair enough. But the problem then becomes, why doesn't the chamber *always* have to provide such inducements to specialize? Why would an individual legislator work to produce a public good otherwise?

A similar worry applies to Krehbiel's argument that legislatures flatly will not commit to organizational forms that encourage distributional losses. One might agree that such "noncommitment is advantageous to a legislative body because it enhances information transmission and precludes distributional shifts." But why would Representative Krehbiel commit substantial time, staff, and energy *first* without knowing whether he would ever see his distributional bonus? For instance, decisions about restrictive rules are highly unpredictable, but more importantly, those decisions are made only *after* the member has gone to the substantial effort of seeking a committee, getting assigned to it, and doing a great deal of bill-specific legislative work. Stated somewhat differently, Mr. Krehbiel shouldn't do "good, hard work" for a firm for future "distributional commissions" unless, at the very least, there is a body of (contract) law which obligates the firm to pay and some mechanism (say, a court) capable of enforcing that obligation. Thus we come back to Olson: the assembly cannot act in its own collective (informational) interest.

These queries notwithstanding, *Information and Legislative Organization* adds an important new perspective to the theory of legislative institutions and provides the most comprehensive empirical assessment to date of alternative theoretical claims. Both theoretically and empirically, then, it offers excellent guidance for the next generation of scholarship. A little distributive pork may be necessary to grease the legislative machinery, but many of us may have exaggerated the distributive (and largely neglected the informational) purposes that machinery serves.

*University of Michigan, Ann Arbor* RICHARD L. HALL

**Let's Make A Deal: Understanding the Negotiation Process in Ordinary Litigation.** By Herbert M. Kritzer. Madison: University of Wisconsin Press, 1991. 203p. $37.50 cloth, $12.95 paper.

The tendency of political scientists to focus on the official output of courts, especially appellate courts, obscures an important reality about litigation in America: the vast majority of disputes never get to court, and the vast majority of those that do get to the courthouse end in a pretrial settlement. Herbert Kritzer asks us to consider the significance of the predominance of settlement in our system of justice, but what engages him is the *process* of settlement in ordinary litigation. The academic world, he charges, does not understand how suits between citizens settle; our knowledge of civil settlement is about what our knowledge of plea bargaining of criminal charges was a generation ago.

In the absence of empirical data, economists and others offer theories of the negotiation process. A few legal writers have documented the give-and-take of settlement negotiations in famous big-stake cases like the Agent Orange litigation. It would be unwise to assume that the bargaining process in ordinary litigation, where lawyers must do a volume business in order to survive, is the same as in the rare megacase that can occupy a lawyer for years.

Kritzer taps a useful source of litigation data, the Civil Litigation Research Project (CLRP), a massive Justice Department–sponsored effort to document grievances and court use in a representative sample of jurisdictions. This material, which Kritzer and a team of other researchers gathered in 1979–80, has provided grist for many studies; but this is the first to focus on the process of negotiation in what can be described as ordinary litigation. These are the relatively low-stake cases that occupy most practitioners on a day-to-day basis. The median amount at stake in the state-filed cases discussed here was $4,500 in 1980 dollars; in the federal courts, the median was $15,000. Most involve injuries or breaches of contracts. Many of the 1,382 lawyers interviewed about their cases were relatively young; a third had been in practice less than five years. Only a fifth received their education at elite law schools.

The most important finding to emerge from this study is that game-theoretic and mathematically derived theories of bargaining do not fit ordinary litigation practice. These theories presuppose a high degree of knowledge about the value of a claim and the costs of pursuing it, information that does not exist in this real-life context. More significant for Kritzer's purposes is the assumption in these theories that negotiators engage in multiple rounds of concessions-oriented bargaining. Kritzer found that lawyers do not spend much time negotiating settlement; they spend less than four hours in most cases. Nor is there much give-and-take: 23% report no exchange of offers, and only 15% recall as many as three exchanges before settling.

Kritzer characterizes this process as "appropriate-result, consensus-oriented negotiation," bringing his findings in line with the empirical literature on plea bargaining, which suggest that lawyers reach consensus quickly, by looking for an "appropriate" disposition. Lawyers may be competitive or cooperative, but they share the goals of avoiding trial and minimizing negotiation time. Clients who want their lawyers to pursue a scorched-earth policy, take note.

Kritzer also finds a tendency for lawyers to evaluate claims in strictly monetary terms. Only 192 of the 1,382 lawyers interviewed believed their cases involved a significant nonmonetary component. Would their clients evaluate these cases the same way? Other research suggest that they might not. Kritzer, surprisingly, does not investigate the lawyer's role in dispute transformation, though he notes that the economics of law practice forces practitioners to cost out cases.

Kritzer's emphasis on the economics of ordinary litigation runs throughout the book, providing an explanation for the low-intensity bargaining that the lawyers report, as well as a guideline for reform. Anyone who seeks to change the system, he suggests, will have to modify the current system of incentives. His data do not permit him to reach the more basic question he posed at the outset, namely, whether we ought to reduce our dependence on settlement. Under what conditions is settlement fair or unfair? Does the lawyer's reliance on settlement tend to alienate citizens who may believe their dispute deserves airing before an impartial tribunal?

Nor do the CLRP data permit Kritzer to explore the changing role of civil litigation in American society. Courts, burdened by growing criminal caseloads, have less and less time to try civil cases. Some states are discontinuing civil trials altogether for long periods of time, a practice sure to slow down the settlement process. Kritzer's data, now over a decade old, is but a one-year slice in this ongoing trend toward the criminalization of the civil courts.

Nevertheless, Kritzer has mined the most comprehensive body of data on civil litigation currently available. He is persuasive in arguing that much of current theorizing about negotiation is inapplicable to the context of ordinary litigation. He suggests another approach that fits his own evidence well and that encourages scholars to make connections between civil and criminal justice in America. This is thus an important (and highly readable) effort to understand at a theoretical level how compromise is reached in civil litigation.

*Syracuse University* Doris Marie Provine

**The Dimensions of Federalism: State Governments and Pollution Control Policies**. By William R. Lowry. Durham: Duke University Press, 1992. 168p. $29.95.

As an integral part of the American political system, federalism has never suffered from neglect from scholars. Yet some students of federalism have been dissatisfied with the largely descriptive nature of the work. Particularly troublesome to some has been the paucity of good theory that can be tested empirically and can be used to predict intergovernmental behavior. While theory associated with fiscal federalism has been tested and proven useful in our understanding of important governmental relationships, political scientists in noneconomic areas have often reverted to descriptive models or, in some cases, partially analytical models that are nonetheless difficult to test empirically.

Thus, William Lowry's book provides a welcome addition to the field, since it attempts to fashion a somewhat modest but very useful framework for understanding why states act as they do in four federal–state pollution control policies: stationary- and mobile-source air pollution and point- and nonpoint-source water pollution. The four were chosen to fit into four cells defined by two key dimensions of the federal policy: the vertical reflecting the extent of federal involvement in the implementation of the policy and the horizontal referring to the level of interstate competition over economic resources evident in the policy.

Along these dimensions, Lowry hypothesizes, state behavior will vary as state programs match severity and response, exceed federal requirements, and share innovations. He provides support for his hypotheses both empirically and through brief case studies of selected states.

Lowry's empirical models build on decades of research on comparative state policy and include measures of severity of needs, pressure of key interest groups, availability of resources, the state's political culture, and federal aid. However, Lowry is primarily concerned with the need variable, especially need as function of two dimensions: the federal role and the potential for interstate competition. He posits that in federal programs with strong federal involvement and low potential for interstate competition, lead states will be more concerned about matching state actions with needs. In programs with low federal involvement and strong interstate competition, there will be little apparent matching between state action and needs. In states falling into the other two categories (where both federal role and interstate competition are strong or weak), the matching will depend on the resources, or wealth, of the state.

Much of the book is devoted to testing these hypotheses, using several dependent variables for each of the four policies and a generally consistent set of independent variables. As is frequently the case when this technique is employed, the results vary when the policy is measured in different ways. However, unlike other policy analyses based on predicting the effect of all or most independent variables, Lowry is interested in only several variables that are relatively stable across the different models.

Unlike many other similar analyses, Lowry does not stop with somewhat nebulous empirical findings. Rather, he considers two additional hypotheses using examples of states that have served as "best cases" in each of the four policies. He hypothesizes that level of interstate competition will affect states' willingness to supersede federal guidelines and that level of federal involvement will affect dissemination and coordination of leading state efforts. To consider these issues, he provided mini–case studies of leading states in each of the four areas. While not, in any sense, providing a test of his hypotheses, the case studies, nonetheless, generally support them.

Lowry proposes in his conclusion that his model can be applied in other areas and is helpful in understanding and predicting policy implementation of local and international policies. Indeed, the generalizability of his model may well be the most useful contribution of the book. The justifications are well grounded in the research and can be replicated. Further, the findings should be of interest to researchers concerned with program implementation even though he does not cite the implementation research and fails to make a connection in his discussion of applicability of his work.

While the book is a contribution to a variety of fields, it is not without flaws. For those not intimately involved in, or interested in, pollution control areas, the book will either be an enlightening educational experience or rather boring. While much of the book is devoted to the empirical testing, this may well be the weakest part of the book. As the author notes, there are measurement problems with several key variables, and the results are far from conclusive. Further, there are some annoying elements, such as confusion as to whether a variable measuring relative contribution of polluting groups, farmers, or utilities to each state's economy is a measure of interest-group strength or need. Other inconsistencies exist with the dependent variable. Expenditure data are sometimes used, sometimes not. Further, in the conclusion, expenditure data are discounted as "symbolic." Slightly more attention to the case studies would have been welcomed.

All in all, the book is carefully considered and well researched and should prove valuable to researchers interested in state politics and policies, federalism and intergovernmental relations, and program implementation.

*Michigan State University* CAROL S. WEISSERT

**Spin Control: The White House Office of Communications and the Management of Presidential News.** By John Anthony Maltese. Chapel Hill: University of North Carolina Press, 1992. 297p. $29.95.

**The Press and the Ford Presidency.** By Mark J. Rozell. Ann Arbor: University of Michigan Press, 1992. 246p. $37.50.

Now that William Jefferson Clinton has found out that the relationship between the American press and the presidency is not so much an adversarial relationship but a symbiotic one, it is important to examine how this relationship came to develop. Two recent books, *Spin Control* by John Anthony Maltese and *The Press and the Ford Presidency* by Mark J. Rozell help us understand this symbiotic relationship between the presidency and the press.

Maltese does a nice scholarly job of detailing the evolution of the White House Office of Communications which was first established in the Nixon administration in 1969. Maltese uses primary interviews with the main players, papers and documents from the presidential libraries and published sources to give the reader a definitive account of the uses and abuses of this office in the attempt to manage news about the president. While presidential scholars in the 1980s documented the increasing tendency of the president to use rhetoric, symbols, ceremonies, and media-events to replace substantive policymaking (see Edwards' (1983) *The Public Presidency*, Kernell's (1986) *Going Public* and Tulis' (1987) *The Rhetorical Presidency*), Maltese systematically outlines the way the Office of Communications tried to manage news about presidents. His strongest chapters are on the Nixon era and the Reagan era of symbolic political manipulation. If forced to choose, Maltese would probably come down on the side of the argument which maintains that the president has more tools at his discretion to manipulate the national agenda than the major news media has to manipulate the images of the president.

He is not as strong in his discussion of the Ford and Carter years. He leaves out any meaningful analysis on the so-called "ridicule problem" during the Ford years. What was the role of the White House spin doctors in trying to combat the perception created by national media that Ford was unathletic, slow, dimwitted, bumbling and prone to mishaps? Carter's problems are reduced to an inability to control images of his administration because his spin doctors could not master the art as well as Reagan's spin doctors. Although Maltese is clearly writing a critique of the Office of Communication by noting the problems it causes for democracy, any presidential candidate and any president reading *Spin Control* would quickly draw the conclusion that they had better adopt the Nixon-Reagan model of symbolic manipulation over the Ford-Carter model of the open presidency. Evidently, President Clinton has made his choice in this regard because of his admiration of Reagan's ability to control information about his presidency.

Rozell represents presidential scholars who think national media have a greater capacity to define the image of the president for citizens than presidents have to manipulate their own images. He continues the research strategy he developed in his (1989) *The Press and the Carter Presidency* by examining what kind of qualitative judgments are made about the president's behavior over the course of his administration by establishment media. He examines how national press views develop over time and supports his analysis by citing passage after passage from accounts in *New York Times*, *Wall Street Journal*, *Washington Post*, *Time*, *Newsweek* and *U.S. News and World Report*. Rozell's well-researched book argues that in the case of a president like Ford, the national news media will fashion the image of the president which will be presented to readers regardless of attempts by presidents to maintain control over mediated political "reality." Rozell's book is a nice companion to the approach presented in Maltese's book.

Rozell provides an excellent qualitative case study of press perceptions of the Ford administration over time, but his case study leaves many questions unanswered and unaddressed. For example, there is no discussion of media responses to assassination attempts against Ford in 1975, and he leaves out media responses to Ford's handling of CIA-FBI revelations in the so-called "Year of Intelligence" in 1975. He details the "Ridicule Problem" in a competent fashion, but he does not explore—among other things—how the Ford team tried to respond to these attacks by having Ron Nesson host "Saturday Night Live".

Rozell could have provided a more quantitative analysis. How does the Ford case study compare to those of other presidencies in terms of positive or negative coverage? His qualitative judgments about press coverage shed little light on issues of intensity, salience and valenced directions of news coverage. Rozell is remiss in providing data about the consequences of judgments by the press. Are positive stories reflected in positive public opinion, and do months of negative stories about an incumbent have any impact on approval ratings of presidents? To be sure, Rozell never intended to write this kind of book, but these are questions which should be addressed. His approach makes for a book which is too descriptive. It contains so many unquotable quotes and citations that much of the book reads like filler.

Rozell is best when he discusses the political and economic environment within which journalists formed expectations about the presidency in the post-Watergate era. Rozell reminds us that journalists have their own criteria for evaluating presidential performance and that they make judgments every day about presidential success, failure, performance, and greatness. Rather than waiting for some academic poll on "presidential greatness" or the analysis of some national public opinion poll on presidential approval scores, journalists make these judgments everytime they write a story. Rozell helps the reader to understand how and why the national press made the judgments they did about Gerald Ford.

As both books demonstrate, the American president needs the coverage of the national news media so that he can spread his political messages to the different power centers in the U.S. democracy. The national news media needs access to the inner circles of decisionmaking in the White House so that they can report on important stories which might sell news products. Both try to manipulate and exploit one another for their own purposes. At this point in the 1990s we have no consistent winners and losers in the game of presidential-press relations, but we have people on both sides who have different levels of playing ability.

*Fairfield University* John Orman

**The Meaning of American Federalism: Constituting a Self-Governing Society.** By Vincent Ostrom. San Francisco: Institute for Contemporary Studies, 1991. 317p. $24.95.

Vincent Ostrom's over three decades of contribution to political theory has had merit in both the consistency of its core and the evolving and expanding insights it has provided on a number of levels. *The Meaning of American Federalism* offers an opportunity to explore the development and application of the structure and logic of his powerful arguments for viewing governance as the basic responsibility of fallible human beings who must, through deliberate thought and continuous experimentation, design and maintain institutions that allow liberty and justice in society. Ostrom has brought together and thematically edited eight previous essays with an introductory and concluding chapter. The book covers ideas that are well known to people who have followed Ostrom's writings. Yet reading the essays will be of profit the anyone—whether familiar with his perspective or not—who has serious concern for the dilemmas and puzzles of determining the "features of a system of governance that would be appropriate to circumstances where people govern rather than presuming that governments govern" (p. 6).

Moving through the essays has the intellectual conviviality of participating in a well-reasoned conversation that touches on a number of topics but is always grounded in a concern for whether and how humans, with respect for one another, can govern themselves. The essays, which are ostensively a search for the meaning of American federalism, are actually a much broader quest for the necessary elements of a self-governing society. Chapters 2–4 do focus on the theoretical base and uniqueness of American federalism during its formative period. However, chapters 5 and 8–10, although drawing upon Tocqueville, Hamilton, Madison, Hobbes, and Montesquieu, go well beyond them in providing a mode of analysis, a critique of the current state of American federalism, and a modern theory of a self-governing society.

Ostrom argues that the key to a self-governing society is a polycentric structure in which there is no monopoly of authority relationships: there can be no single point of dominance. For conditions of equality in liberty and justice to be met, individuals must be of equal standing with one another in both the norms and laws of the society. Spontaneity is necessary in the sense that polycentric orders are open, self-organizing, and self-governing systems in both the private and public sector. However, the establishment and maintenance of such arrangements requires "a culture of inquiry and problem solving that has error-correcting potentialities," or what is called "embodied intelligence" (p. 218). It is not governments that govern but mortal, fallible humans, who must make reflective choices among understood alternatives in an uncertain world. Ostrom underscores the point that history provides ample evidence of the difficulties in achieving polycentric orders and, when self-governance is gained, of the continous threat of oligarchial and centralized dominance in society.

In addition to constructing and elaborating on this polycentric model of governance from the American experience, Ostrom applies it as a template to assess three quite current issues in governance. Chapters 6 and 7 draw together the core of an influential set of ideas calling for metropolitan areas to be understood as polycentric systems of governance and the provision and production of public goods, as occuring within a public economy. (Ostrom himself was a leader in developing these ideas.) Chapter 5 focuses on the 1985 decision of the U.S. Supreme Court in *Garcia* versus *San Antonio Metropolitan Transit Authority* but is a more general critique of current tendencies toward centralization of power in the national government and the executive. Even more recent events and revelations of the use of power during the Reagan and Bush administrations make Ostrom's analysis of the current state of American federalism in this essay particularly relevant. He warns that "unless citizens can hold officials accountable so that they perform within the bounds of fundamental law, . . . democracy no longer exists and autocracy prevails" (p. 130). He concludes that federalism (a polycentric and self-governing system) has a future—but whether in America or not is problematic. Further, he notes that the many opportunities creating self-governing societies elsewhere in the world at this historical point cannot be realized under the dominance of American leadership. Other nations "may draw upon ideas associated with American federalism, but those ideas must become a part of the cultural heritage that different peoples draw upon in shaping their own embodied intelligence" (p. 270).

Whether one agrees with the theoretical framework and conclusions or not, the generic questions of governance that they address, the logical force of the arguments, the ranging command of history, and the opportunity to observe the crafting of this complex perspective over time make the book well worth reading.

*University of Delaware* Robert Warren

**The Rise of the Therapeutic State.** By Andrew J. Polsky. Princeton: Princeton University Press, 1991. 287p. $35.00.

Issues regarding how society treats citizens deemed at variance with commonly held norms of behavior are addressed by virtually all of the social sciences on a recurring, sometimes frequent, basis. So it is with the poor. Among the questions at the heart of this discussion are "Where does the fault lie?," "Do individuals or social institutions need to be changed?," and "How do we trade off individual freedom and the public's collective interest?" The central theme of Andrew Polsky's monograph is that the American experience has come to be dominated by a set of professional and bureaucratic interests embodied in the framework of social casework. This "therapeutic" approach assumes people exhibiting aberrant behavior do not have the right value structure to achieve self-sufficiency, healthy interpersonal relationships, and positive self-esteem. Professional intervention must be introduced to diagnose the individual pathologies causing these behavioral problems and then help correct them. It is up to "normalizing institutions" (caseworkers, clinicians, probation officers, and the like) to facilitate the curative process.

Polsky comes to some appropriate and important critical judgments about the therapeutic framework. Perhaps foremost among these is the fundamental fact that most of the helping professions' interventions in the areas of poverty and crime simply have not had the desired impact (at least not in a significant aggregate sense). Polsky's judgment is that both the methods used and the clients have contributed to this "wretched performance" (p. 5). Among the former are problems of professional and bureaucratic self-interested behavior, a lack of coherence among practitioners and programs using this approach, and the decreasing confidence that we know how to solve the problems of poverty and violence. Clients, on their part, have not cooperated very much in their relationships with the professionals, often trying to manipulate the caseworkers or bureaucracies for their own purposes. Even where programs have obviously failed to meet their objectives, the therapeutic professionals have insisted that their disciplines can come up with the solutions to the problem. Indeed, they recommend still other methods be explored, new demonstration programs launched. Polsky rightly asks how, despite such obvious failure, the therapeutic state still continues without major challenge.

To students of the history of poverty and poverty programs in the United States, much of this will seem familiar. Though nicely developed, the critique of the therapeutic state might be seen as one among other arguments for the "institutional failure" explanation of poverty and deviance. The extensive corpus of work growing out of the nation's experience with Lyndon Johnson's War on Poverty touches on many of the themes raised in this book. Ironically, what is most problematic about Polksy's effort is his suggestion about what better direction might be followed to remedy the social ills of concern today. In addition to guaranteeing a decent minimum income, he recommends that a "political approach to social membership would require a far-reaching political reorganization, including sweeping decentralization and community control" (p.218). Here we have the language of community action in the 1960s and 1970s. Direct participation by community residents through frequent public meetings and community mobilization on important issues would be the alternative to individual therapy. This would provide a path to social membership and individual freedom. Yet, such benefits are based on imagination, not experience. While community action measurably affected a host of urban institutions, the effect was incremental and there is little evidence of any lasting impact on the poor themselves. As Polsky points out, " . . . we must acknowledge in all candor that we have little concrete evidence to support the proposition that political mobilization can integrate marginal groups into the social mainstream, much less bring about desirable behavioral changes at the individual level" (p.217). It is interesting to note, however, that the term "empowerment" has been adopted by some conservatives (as well as liberals) who suggest self-governing as an alternative to more centralized bureaucratic control.

There is, of course, a possible argument in favor of the notion of therapeutic intervention. Millions of citizens spend their own money on counseling of different types. Financial capacity does empower consumers to choose for themselves the intervention they deem effective (regardless of its actual impact). One might argue that services have failed because they have been grossly underfunded. Indeed, the public-sector budget for the helping professions is trivial next to the resources spent on entitlements for the elderly and retirees. As with community action, however, one would have to accept on faith the notion that, properly implemented, the therapeutic state would alleviate the deep-seated problems of such concern in our society.

*University of Rochester* BRUCE JACOBS

**The New Institutionalism in Organizational Analysis.** Edited by Walter W. Powell and Paul J. DiMaggio. Chicago: University of Chicago Press, 1991. 478p. $65.00 cloth, $24.95 paper.

This volume celebrates more than a decade's successful effort to breathe the new life into the institutional analysis of formal organizations. It reprints four now-classic foundational articles and a new set of conference papers (written almost entirely by sociologists) about equally divided among general theoretical statements and empirical case applications. The neoinstitutionalist approach diverges from a predecessor version, exemplified by Philip Selznick's investigation of how the Tennessee Valley Authority coopted local communities to implement its rural electrification agenda. While similarly rejecting the rational-choice underpinings of institutional economics and positive political theory, the new sociological institutionalists initially downplayed political conflict in favor of shared norms. Customs, obligations, rules, decision routines, and similar consensual assumptions were woven into comprehensive explanations of how macro constraints became taken-for-granted beliefs among participants. Drawing from cognitively informed theories of social action, neoinstitutional analysts coupled their insights with culturally and historically based organized logics to generate richly insightful research agendas.

Walter Powell and Paul DiMaggio's introductory essay provides a useful overview of the origins, development, and prospects for the contemporary approach. Its earliest formulations were strongly shaped by enthno–methodological, phenomenological, and Parsonsian action–theory elements. These accounts emphasized the micro symbolization and socialization processes through which organizational participants come to adopt the normative standards that sustain macro structures. A major conceptualization of this era (launched in the reprinted articles by John Meyer and Brian Rowan and by DiMaggio and Powell) was the strong impetus from such processes toward homogeneity among organizations operating within societal sectors. Political and cultural expectations held by state and professional agents about conformity to proper organizational structures compelled an increasing isomorphism, at least at the surface impression–management level. Organizations are rewarded with material support and ideological legitimacy for adhering to technical, structural, and behavioral standards, regardless of their actual performance efficiencies. The range of phenomena staked out for treatment span from by Lynne Zucker's examination of cultural persistence in the transmission, maintenance, and resistance to change within small experimental groups to W. Richard Scott and John Meyer's propositional delineation of entire societal sectors. The latter note that the increasing national centralization of decisions and funding has reduced the diversity of forms that has always been a source of revitalization within federal bureaucratic administrative systems.

As the maturing neoinstitutionalism began encountering empirical conundrums, analysts were forced to reconsider their tendency to presume a conflictless imposition of socially constructed uniformities on organizational actors and their interests. As they grew to appreciate how ambiguities, tensions, and contradictions could stimulate organizational change, neoinstitutionalists found room once again for power, the central theme in Selznick's earlier version. Thus, the five new theoretical statements in this volume each grapple, to some degree, with such inherently political questions as Who gains and who loses by maintaining institutional arrangements? When do challenging collective movements succeed in reconfiguring the rules of the game? and When does the external polity (represented in the state's legislative, judicial, and executive organs) intrude effectively on the civil society's institutional order? Ronald Jepperson argues that institutions not only constrain formal structures but "simultaneously empower and control" their participants (p. 146)—a point that he and Meyer elaborate while charting how a universal polity of modern organizational forms rapidly diffused among national societies. In contrast, Powell points to the contradictory and overlapping demands of multiple levels of government in the American polity as an important source of resistance to institutional conformity pressures. Scott notes that superordinate units frequently authorize or legitimate their subordinate units' forms, rather than coercively imposing such constraints. In the most explicitly political analysis, Roger Friedland and Robert Alford offer a politics of institutional contradiction, in which struggles over the appropriate logics (especially markets versus state regulation) "alter the institutional relations constituting a society" (p. 256). These essays move the field toward greater sophistication and more nuanced answers to basic political questions.

The six empirical chapters illustrate both the potential and the limitations of the contemporary revival. Their substantive range is an impressive as their methodological creativity: the professionalization of art museums in the United States (DiMaggio); the spread of Minneapolis corporate philanthropy (Joseph Galaskiewicz); the historical diversification of large U.S. firms (Neil Fligstein); the transformation of the community college educational mission (Steven Brint and Jerome Karabel); the creation of East Asian business enterprise groupings (Marco Orru, Nicole Woolsey Biggart, and Gary Hamilton); and the birth-and-death dynamics of Toronto social service agencies (Jitendra Singh, David Tucker, and Agnes Meinhard). However, these analyses largely focus on highly aggregated organizational or interorganizational levels of analysis, giving only sproadic clues to their linkages with lower-level work groups, employees, clients, customers, suppliers, and general publics. Thus, the claim by DiMaggio and Powell that "actors and their interests are institutionally constructed" (p. 28) awaits empirical confirmation by the next generation of researchers, who should explore vertical influences extending in both directions.

Political scientists will find the neoinstitutional perspective most pertinent for developing an explanation of public bureaucracies even though this organizational type is largely neglected here. As disenchantment with wholly rational-choice accounts of the public sphere grows, the new institutionalism should prove a fruitful source of alternative ideas from which to construct a meaningful politics of organizational behavior.

*University of Minnesota* DAVID KNOKE

**The Third House: Lobbyists and Lobbying in the States.** By Alan Rosenthal. Washington: Congressional Quarterly, 1992. 242p. $27.95 cloth, $17.95 paper.

Interest group scholars have begun to pay increasing attention to patterns of lobbying and representation in the 50 states. Projects by Ronald Hrebenar and Clive Thomas, and by Virginia Gray and David Lowery are among those which have pushed scholarship in this area forward. Alan Rosenthal's fine new book, *The Third House,* is a welcome addition to this growing list of work on interest groups at the state level.

Rosenthal's data come from interviews with 105 lobbyists in six states (California, Colorado, Florida, Minnesota, New Jersey, and Texas), yet it was not his intent to fashion a strict comparative study. Using unstructured interviews he follows different lines of inquiry with his subjects, pursuing answers to two general questions: "Just what do lobbyists do?" and "How do they do it?" (p. 9).

Rosenthal uses his interviews to great advantage in constructing a tightly focused analysis of the strategy and tactics of legislative lobbying. He offers readers a rich, thick description of how lobbyists do their job of trying to shape public policy. I don't know of anyone who has done a better job of getting inside the heads of lobbyists, detailing how they make their decisions in the rough-and-tumble of state politics. This is a highly sympathetic study of people in a much maligned profession, and the book is written largely from their point of view.

In comparison with Washington lobbyists, there are some striking differences in the way state house lobbyists operate. At the state level, for example, lobbyists place much more emphasis on friendships and building personal relationships with the legislators they must try to sway. As one Florida lobbyist told Rosenthal, "Everything must be seen in terms of relationships" (p. 112). *The Third House* is impressive in its documentation of how much effort state lobbyists put into developing these bonds of friendship and in analyzing how central these endeavors are to the strategy of influence. A good part of what these lobbyists do is devoted to entertaining, giving gifts, taking legislators to basketball and football games, and arranging hunting and fishing trips.

Although Washington lobbyists try to build relationships with legislators, their focus is less on making friends and more on developing a reputation for trustworthiness and in gaining respect for their knowledge of the substance of public policy. The thousands and thousands of lobbyists in Washington (and a stricter ethical climate) make a strategy built on forging friendships with legislators impractical. The role of PAC contributions and campaign finance, however, seems to be depressingly similar on both the national and state levels.

One area where the increasing professionalization of Washington lobbying is being emulated at the state level is in the growth of the full-service lobbying firms. These organizations can provide a full range of advocacy tactics and communications services for clients who need much more than a representative to talk to legislators for them. Rosenthal is careful throughout the book to distinguish between lobbyists who are full-time employees of an interest group, and contract lobbyists who work on behalf of a range of clients.

Rosenthal's decision to examine lobbying from the point of view of his subjects pays some handsome dividends, but he carries this approach a bit too far. Like his lobbyists, Rosenthal accepts the system pretty much as it is. Even though he delineates many questionable ethical practices, Rosenthal is steadfastly non-judgmental. He's clear about the tie of campaign contributions from lobbyists and their PACs to increased access to legislators but is largely uncritical of it. More broadly, the question of who is—and who is not—represented at the state house bargaining table is given scant attention.

Although he has left some important questions about representation and lobbying in the states unanswered, Rosenthal's book makes a valuable contribution to the literature on interest groups. Scholars who focus on lobbying at the national level will find this book of interest as it paints a vivid picture of a much different kind of policymaking system than the one found in Washington. Those who write and teach about state government will find *The Third House* to be an especially helpful guide to the operations of state legislatures. This is a highly readable book and students will enjoy it too.

*Tufts University* JEFFREY M. BERRY

**Common Law and Liberal Theory: Coke, Hobbes, and the Origins of American Constitutionalism.** By James R. Stoner, Jr. Lawrence: University Press of Kansas, 1992. 287p. $35.00.

*Common Law and Liberal Theory* is a welcome addition to the sclerotic literature on the origins of judicial review. It offers a fresh perspective, for we have something to learn about the beginnings of judicial review by seeing it within the common-law tradition. Stoner insists that the current debate over judicial review is little more than a caricature of academic analysis. Contemporary liberals, he argues, have advanced theories that stress that judicial review is legitimate only to the extent that it rests on individual and moral rights, making it, in the end, an engine for the perpetuation of individual interests over community interests. Modern conservatives, on the other hand, seek constitutional stability by turning to a jurisprudence of original intention, in which texts are interpreted according to the expectations of their framers. The former, Stoner observes, suffers from the defect that morality based on judicial whim is no morality at all; the latter pales before the paucity of sources to reconstruct the framers' intentions, let alone their motives, authoritatively.

Stoner appreciates that no matter how stale the current debate, the institution of judicial review remains worthy of our attention, since it is one of the most important ways in which the constitutional system reconciles popular sovereignty and fundamental law. To explain its origins, therefore, he turns to a sophisticated reading of major seventeenth- and eighteenth-century legal and political texts. Stoner observes that the practice of judicial review, at least as it was understood up to the time of the founding, was rooted in distinctly different and, in some respects, quite antagonistic sources. One of these was the English common-law tradition associated with Sir Edward Coke, the seventeenth-century judge and parliamentarian who authored *Dr. Bonham's Case*. Stoner revives our appreciation for *Bonham's Case* as an important milestone in the development of the common law. Coke not only was well known to Americans of the founding generation but was read by them as an authority on the proper interaction among judges, law, and politics. Stoner insists that Coke in *Bonham's Case* assimilated right reason, precedent, and local tradition into English law as a whole. He was, in short, a bad historian but a great common-lawyer. Modern liberal scholars, such as Bernard Bailyn and Gordon Wood, have missed this important point, because their disappointment with the discovery that *Bonham's Case* did not protect individual liberty through the aegis of judicial review blinded them to its larger significance. Coke's achievement was in asserting the primacy of law over politics and the unity of English law as a matter of history. In doing so, Stoner argues, Coke established a powerful tradition that connected judicial review not to the protection of individual rights but, instead, to the maintenance of community.

Stoner, however, does not swallow Coke whole. The common-law tradition alone could never support judicial review, since its "weakness" was its inability "to start fresh—or to meet a drastic change in fortune," such as the American Revolution (p. 68). The framers of the American Constitution, therefore, necessarily drew from the liberal political theory of Thomas Hobbes, who was at war with most of Coke's ideas. Stoner finds unity in disharmony by boldly connecting the two, doing so through the medium of those who subsequently interpreted them: John Locke, Montesquieu, and William Blackstone, all of whom had a more direct impact on the founders than did either Hobbes or Coke. Taken together, Stoner argues, it was the common-law tradition of Coke and the liberalism of Hobbes and his intellectual successors that explains the origins of judicial review. Stoner insists that the recovery of liberal forms of political thought (the major achievement of Bailyn and Wood) is insufficient to explain why the framers gave the judicial branch the capacity to expound a constitution that rested so fully on popular sovereignty.

Perhaps the most important point to make about Stoner's argument is what it is not meant to do. It does not, like so much recent scholarship, seek to answer the question of whether the development of judicial supremacy has been legitimate. The book ends with a desultory discussion of *The Federalist* and the Bill of Rights, leaving others, such as Robert Clinton and

Christopher Wolfe, to sift the Supreme Court's record for evidence about whether it should be the final interpreter of the Constitution. Instead, Stoner effectively addresses the badly neglected question of what we expect of judges in a political system that rests on popular sovereignty and a legal order committed to the idea of fundamental law. Stoner adds to this debate by arguing that to ignore the common-law origins of American constitutionalism is to overlook how important history, collective wisdom, and responsibility are in comparison with the liberal emphasis on individual rights and a scientific approach to their protection through judicial action.

*University of Tulsa* KERMIT L. HALL

**The Disappearing American Voter.** Ruy A. Teixeira. Washington: Brookings Institution, 1992. 242p. $31.95 cloth, $12.95 paper.

Revising and extending his research on patterns, causes, and consequences of low voting participation in presidential elections, Ruy Teixeira examines voting turnout from 1960 to 1988. The focus is on why turnout rates were so low during that period, the extent to which bias in electoral outcomes and policy outputs is created by low turnout rates, and the potential effectiveness of efforts to lower the perceived costs and increase the perceived benefits of voting. The analysis is based primarily on the Bureau of the Census Current Population Surveys from 1972 to 1988 and American National Election Study data from 1960 to 1988. The thesis developed is that turnout is low because of high costs to the individual of voter registration and the low perceived benefits derived from participation, the latter mostly attributable to the structure of electoral competition and weak party mobilization.

As with any study of turnout, the available data impose limits on the hypotheses which can be tested. The quality of political information available to respondents, the extent and nature of citizens' social connections which might contribute to political mobilization, the nature of other political contests—both for offices and on ballot referenda—all may influence turnout and mitigate or enhance the effects of other variables on turnout. Appropriate measures for these types of variables are either not in the data sets the author used or limited in their quality or the time period covered.

As others have concluded, Teixeira finds that increases in employment in higher status occupations and rising levels of income and education created the potential for higher levels of turnout, but the effects were negated by decreases in political involvement and efficacy and declines in social connectedness (as measured by changes in age distributions, marital status, and church attendance). The author's efforts to capture the effects of declines in other forms of social connectedness produce non-significant results. However, the author is too ready to accept the conclusiveness of results based on the less than optimal measures of these variables available in the data sets. There are also problems with his conceptualization and measurement of the effects of political mobilization by party organizations and with inferences made concerning the contributions of political demobilization and disconnectedness to turnout decline.

Teixeira argues that political disconnection played a larger role than did the decline in social rootedness in the downward trend in presidential election turnout. Testing for class bias in turnout declines, he concludes that the drop was somewhat more rapid among those groups least likely to vote at the beginning of the period studied. Several cautionary notes should be kept in mind when evaluating Teixeira's conclusions. First, many of the measures of independent variables are less than optimal. They are in many instances all that are available as indicators of key concepts, but that does not obviate the fact that inferences made should be interpreted more cautiously. Teixeira occasionally overgeneralizes from the results of his data analyses, as for example when he concludes that non-voting makes little difference in election outcomes and in policy outcomes as well. The data analysis examines only voting turnout in presidential elections and a very limited set of policy issues.

Examining the potential effects on turnout of lowering individuals' costs of voter registration through several reform proposals, Teixeira estimates that no matter how extensive these reforms, their collective effects would increase turnout only from 8 to 15 percent. Turning to the alternative of increasing citizens' perceived benefits from voting, he argues that the key to increasing turnout is enhancing the links between political elites and the mass public. A number of possible approaches to increasing citizens' motivation to vote are considered. These involve changing factors which affect either cost or benefit calculations, such as compulsory voting, a unicameral Congress, and multimember districts. Teixeira rejects all as infeasible. The motivational cure he proposes is reconnecting Americans to politics. Methods examined which might achieve such a result include changes in how campaigns are conducted and in how politicians, political parties, and the media organize their relationships with citizens.

While there are problems with data and interpretation, Teixeira's book is nonetheless a valuable contribution to the continuing debate on the causes, consequences and effects of low voter turnout rates in the United States.

*University of Florida* M. MARGARET CONWAY

**The Search for Rational Drug Control.** By Franklin E. Zimring and Gordon Hawkins. New York: Cambridge University Press, 1992. 219p. $24.95.

Zimring and Hawkins offer a devastating critique of the premises and tactics of U.S. antidrug policy. The authors begin by outlining three different ideologies of drug control (legalists, public health generalists, and cost-benefit specifists), but it is quickly apparent that the key struggle in this work is between the misguided but predominant legalists and the clear-thinking cost-benefit specifists.

"Legalism" is the prevailing ideology of drug control in federal policy. Legalists see illegal drug use as a moral failure and a challenge to the authority of the state. These driving but blinding ideological premises bring the legalists to a long series of errors in the formation of antidrug policy. Some of the most fundamental mistakes surround the very definition of what the "drug problem" is. Disregarding both science and common sense, legalists insist that there is an important difference between illegal drugs and the often more damaging and costly legal ones like alcohol and nicotine. Legalist "drug" policy deals strictly with the former set. Another basic error is the collapsing of all illegal drugs in to one homogeneous category, so that, for example, the nonaddictive and relatively benign drug marijuana is lumped together with a dangerous and addictive drug like crack cocaine. Legalists also fail to distinguish between drug use and drug abuse. They often see the casual and occasional user as a greater problem than the hard-core addict. In short, governed by the simplistic and misguided terms of this ideology, the legalist architects of current antidrug policy eschew any real effort to understand the drug problem or develop a rational drug control policy.

The eclectic but consistently critical first section of the book includes two other discussions that are required reading for anyone interested in drug policy. The first is an invaluable critique of the measurement problem. How many Americans use drugs? How costly is this drug use? How much use is really problematic enough to merit government attention and expenditure? The authors' answer is that we do not really know. The extent and cost of something as secretive and multifaceted as illegal drug use is simply very difficult to measure in a mass society. While the authors feel that more promising measures are emerging, it is clear that billions of dollars are being pumped into policies based on no real understanding of what these policies are supposed to solve or how we can evaluate them.

The second discussion is an intriguing critique of the decriminalization debate, which argues that the participants are asking questions that are both wrong and unanswerable. The fundamental complaint of this excellent chapter is that drug policy should not "trickle down" from sweeping beliefs about liberty and order and grand policies like legalization or zero tolerance. Rather, using the cost–benefit specificity of the authors, we should take a drug-by-drug, issue-by-issue approach that carefully delineates the gains and losses of any particular policy.

While the main strength of this book is its critique, the authors do make some proposals for reform that amount to a call for the wider use of cost–benefit specificity. Such an approach leads to the conclusion that the main goal of drug policy should be damage reduction, not law enforcement for the sake of legal authority. Under this framework, policy formation should begin analyzing the costs and benefits of different types of drug use, as well as the costs and benefits of specific policy responses. The protection of children emerges as the central goal; prevention of predatory crime is important but not clearly linked to drug control; marijuana laws should be relaxed but not eliminated. The clearest recommendations are largely procedural reforms that amount to a boilerplate application of mainstream public policy rationalism. While reasonable, they are certainly not earth-shattering revelations. One must also wonder whether the panacea of rational decision making is a promising cure for a policy area that is so fundamentally constituted by irrational fears and reactions.

If they can work with an occasional sense of chaos created by the effort to cover too much too quickly, readers will find a wealthy source of information on what is wrong with drug policy in America. The critical aspects of this book are its strength and are required reading for scholars in this area. In one area, however, the authors have not carried the critical effort far enough. This is seen in their tendency to depict public concern over illegal drugs as both a driving force behind policy and "an authentic grass-roots phenomenon of the mid-1980s." There is good reason to doubt whether authentic grass-roots phenomena occur at all in this mass-mediated culture; and it is clearly the case that politicians and the news media have (despite generally declining use rates) been fanning the flames of the "drug crisis" since the early 1980s. In this context, it is a *massive leap to depict public fears about drugs as either* a grass-roots phenomenon or a driving force. Since the authors so successfully problematize other elements of the war on drugs, it is a dissappointment that they appear to accept its very origins as a simple case of public concern being met by a responsive democratic state.

*Ohio University* JOHN GILLIOM

# COMPARATIVE POLITICS

**Comparative Health Policy and the New Right: From Rhetoric to Reality.** Edited by Christa Altenstetter and Stuart C. Haywood. New York: St. Martin's Press, 1991. 332p. $59.95.

**Managing the Medical Arms Race: Innovation and Public Policy in the Medical Device Industry.** By Susan Bartlett Foote. Berkeley: University of California Press, 1992. 285p. $35.00.

**Technology and the Welfare State: The Development of Health Care in Britain and America.** By Stephen Uttley. New York: Routledge, Chapman & Hall, 1991. 213p. $19.95 paper.

The single most problematic area of social policy to face each of the advanced, industrial democracies as they prepare to end the century is health care. Both Susan Bartlett Foote and Stephen Uttley show how technological "progress" has gone hand in hand with the development of sophisticated health care systems in these countries. So it is not the quality of health services available (as Foote's fine monograph on modern medical devices demonstrates) but the cost of health care and of access to it that is nettlesome to all the policymakers that oversee the health care universe. Insofar as resources are finite, as they are in each country (despite their relative wealth) and insofar as the unfolding of the Twentieth century has been a history of increasing demands, from welfare to warfare, on each state, then the costs of providing sophisticated health services to an extensive population must be one paramount concern of each policy system.

Each system, of course, responds to the problematic of health care slightly differently. In particular, as Uttley demonstrates in his comparison of Britain to the United States, some nations have chosen to trade access for cost. Yet the basic imperative is everywhere the same: provide high quality care, do so as cheaply as you can, and make it available to as many as possible.

The uniqueness of the systems lies in how each has chosen to play these three missions off against each other. The United States has historically stressed high-quality care. This has often taken the form of high-techology rescue medicine, embodied in the "medical arms race" that Foote describes. Equal access to the system has, however, been significantly sacrificed. Other systems, like those in Europe or Canada, have downplayed high-technology medicine in order to buy more care for more people—access. Yet even in these systems, which typically spend far less on health care than the United States (an average of about 7% of gross domestic product for the former, compared to over 12% for the latter), cost imperatives come constantly to the fore.

Hence, recent decades have witnessed a number of innovative policy proposals designed to contain health care expenditures. In particular, many of these have come from the political New Right (from North America to Europe), which has stressed the use of market-based mechanisms to restrain health care costs. The Altenstetter and Haywood volume describes these for a number of countries and analyzes how these proposals have fared in the transition from the level of rhetoric to the reality of implementation. The decidedly mixed experience of these conservative, market-based reform movements, we see, only reinforces the problematic—indeed, intractable—character of health policy reform in the advanced industrial democracies. Moreover, we learn from all three of these books that reform in the most problematic system of all, the American, is likely to be fraught with false starts, delay, ferocious fights, and incrementalism as the new Clinton administration tries to move American health care toward the Twenty-first century.

The finest book under review here, Foote's *Managing the Medical Arms Race,* is a superb study of the political economy of the medical device industry, turning a powerful analytical lens on a critical component of the health policy universe in the United States. Foote describes in complex but very plain detail how the medical arms race is part and parcel of the fragmented and incrementalist universe of the American political system.

The major contours of this story will not surprise political scientists. The interaction between the private sector of medical device inventors, manufacturers and marketers, and a disjointed government whose myriad institutions frame a highly contradictory incentive structure leads to policy schizophrenia. For example, the National Institutes of Health, through a variety of targeted disease programs and through additional programs that target specific device technologies (such as artificial hearts), encourage the research and development of higher-and-higher-technology medical devices. Other government programs, such as Medicare, increase access to high-technology services, as do policies designed to subsidize the hospital infrastructure of the country. The result here, of course, is significantly to increase the diffusion of new medical devices that are developed. Then, naturally, other government agencies weigh in (too late, probably) to restrain the wild growth in these devices. In particular, Foote argues, one major mission of the Food and Drug Administration in its licensing role is to restrain the availability of these technologies, which are inevitably very expensive.

No short summary can do good justice to Foote's solidly researched, tightly argued book. And while the story of the medical arms race—an uncoordinated, disjointedly incremental policy universe—will sound familiar to political scientists, it is nonetheless a story of compelling policy relevance. Foote will accomplish a great service in educating public and private actors to the perverse incentive structure at play here and both the good and bad consequences of it. Moreover, the excellent scholarship on display in this book makes it quite appropriate for classroom use both in health-oriented curricula and more standard political science courses.

Stephen Uttley's *Technology and the Welfare State,* unfortunately, does not reach the high standards of readability and immediate policy relevance set by Foote's book. In a dense and pedantic analysis, Uttley attempts to describe the evolution of the modern health care systems of Britain and the United States against the backdrop of theories and controversies about the welfare state in general. To be sure, there are a good many relevant points to the Uttley book. The interaction of technological development and public and private health markets has had great impact on the evolution of many low- and high-order technologies. The book sets out these processes of technological evolution in terms of the complex interplay between phases of innovation and categories of technologies. The former follow a progression that begins with invention (research), proceeds to application (development), and concludes with diffusion (wide marketing). The latter divide the universe of health care technologies into those that diagnose, those that treat, those that maintain (health) and prevent (illness), and those that improve the organization and delivery of technologies.

Uttley's portrait of the development of the British and American health care systems points up the long-standing role of relatively coordinated government initiatives in organizing and reforming health care in the British case, compared to a rather uncoordinated, Johnny-come-lately government in the American case. This finding will surprise few. However, Uttley argues, there is now some important convergence between the two. The period since the 1960s, in particular, is characterized by a much more active government role in the United States. Recent events in the British system, on the contrary, have emphasized taking the government back out of health care, to some extent, especially through the contracting out of various services and through the growth of private health insurance. However, the general theme of market-oriented reform in Britain is much more thoroughly explored in the edited volume.

Despite the extensive analytical and historical look at the links between technology, governments, health markets, and the development of the welfare state, Uttley neglects two particularly important points emerging from the British–American comparison. First, in spite of the growth of the American federal role and some diminishing of the British, the differences between the systems nevertheless dwarf any convergence. The National Health Service remains an extremely centralized system, tightly coordinated by the British government. Privatization, in any of its forms, has so far only taken place at the (relative) margins, as Ranade and Haywood point out in the edited volume. The American system, however, as Foote shows, remains highly decentralized, uncoordinated, and private-sector oriented, at least for now.

Second, insofar as medical technology is concerned, Uttley does not recognize the crucial difference between ordinary technologies (or low-order ones) and extraordinary technologies (or high-order ones). Whereas the former are made extensively available to British citizens by the National Health Service, that is, not rationed, the latter technologies are tightly rationed according to "need" criteria. In the United States, the extraordinary are, indeed, rationed, mostly by ability to pay. Hence, the prominence of rescue medicine for those that are insured, that is, have the ability to pay. The ordinary, however, are also rationed in the United States. This is the problem of access. These differences between Britain and the United States mean that the dilemmas facing policymakers, the constraints upon providers and patients, the fiscal pressures upon payers, and the whole political discourse about what to do with health care policy, is deeply divergent in the two countries.

The edited volume sets as its task the examination of neoconservative political responses to the dilemmas of health policy and how these responses have fared in the face of the reality of implementation in a host of countries, mainly the advanced industrial democracies; that is, while conservative governments (and, as Paul Godt shows in the French case, ostensibly socialist governments, as well) have sought to liberalize, deregulate, and privatize components of the health care system, results in practice, have been more than problematic all around. Governments have followed (more accurately, talked) a neoliberal line not only in voicing the ideological position that less government, in *any* domain, is better government but, perhaps more importantly, in grasping for new means to deal with intractably old problems—the high costs and frequent inefficiencies of modern health care systems.

Some of the chapters in this book are, unfortunately, completely irrelevant to the ostensible theme of how the rhetoric of the New Right has fared in the fires of real politics in various nations. (The Björkman chapter, which concludes the volume, is particularly egregious in this regard; the editors should have been more careful.) Moreover, the introductory chapter by the editors does little to specify a common framework (which is not followed in any case) or to assess the theoretical literatures of ideology and conservatism. Nonetheless, most of the book *is* characterized by solid, sometimes brilliant, empirical scholarship that does much to enlighten our understanding of how ideology confronts reality in complex health policy systems—all this in a compelling comparative context.

Richard Saltman's chapter on the Nordic countries is an exemplar. Against the backdrop of the traditional "Nordic model"—tax-based funding, universal access, public hospitals, and decentralized munipal or regional administration—Saltman explores the changing dynamics of health care politics in the 1980s. More prominent resource constraints and changing assumptions about resource distribution challenge the status quo of health system finances; and Nordic citizens have become more insistent at demanding user-friendly, not merely competent, clinical care. The traditional command-and-control health care model has thus given way to an emphasis on less rigid, more innovative public-sector techniques.

Thus, Saltman reaches a conclusion about the public–private mix in Nordic health care similar to that reached by Godt for France, Webber for (West) Germany, Ranade and Haywood for Britain, Brandon for the United States, Manga and Weller for Canada, and Steslicke for Japan, pointing to a remarkable comparative convergence. In the Nordic case, Saltman argues, there is no correlation between support for specific privatization policies, on the one hand, and a conservative or neoliberal political orientation on the other. Sweden (under Social Democrats), for example, experimented with some contracting out; Denmark (under a conservative government) did not, nor did it attempt to engage in any other neoliberal practices. Regardless of government ideology, therefore, in practice Nordic health policy systems concentrate on a wholly different dimension of reform, namely, how to reconstruct publicly operated, tax-based systems for a "second generation of service." The question is therefore not whether public-sector health systems will survive but, rather, what they will look like.

The irrelevance of ideology in the health policy domain is reinforced by almost every other country study in this volume. Godt shows how the socialists in France had already began to tighten administrative incentive structures to make administrators and physicians more bottom-line-oriented. The conservative government of 1986–88 continued this line but did not significantly privatize or otherwise reform the major public–private character of the system. Webber shows that the conservative German governments since 1982 have faced almost impossible political obstacles to reforms designed to regain a greater measure of control over the health care system and that in their attempts to reform, there was very little designed to substantially reduce the role of the state and increase the scope for the operation of market forces (the classic neoliberal agenda).

Likewise, in the study of Canada, Manga and Weller argue that the Conservatives' main goal was simply to shift much of the cost of care onto private consumers. Even in this, they have been mostly ineffective. Even in the British case, Ranade and Haywood suggest that in spite of some important changes in management practices, accountability mechanisms and some modest growth of the private sector, "the general picture is in great measure one of policy continuity" (p. 106). There have been no drastic changes either in methods of funding and provision or in the basic structures and dominance of the National Health Service. Fundamental principles of the system have been reaffirmed many times throughout the Thatcher reign.

Other studies from this rich volume are worth mention. Brandon, for example, highlights superbly how the Reagan rhetoric came into play on moral issues such as abortion, all the while that a substantial growth in the government's role in this diffuse health care system was taking place, especially in hospital and physician payment systems for Medicare, policies that served then as important benchmarks for the private sector. And Steslicke demonstrates how, in that quintessentially conservative case in almost every political respect—Japan, as governed by the Liberal Democratic party—policymaking in health care has moved more and more to strengthen the government's control over the critical levers of a publicly mandated but largely privately run system.

In one important way, all three volumes are characterized by diagnosis and evaluation. They agree that modern, sophisticated health care systems have been on the horns of a dilemma for some time. The main focus of the dilemma in these works has been fiscal. The systems all cost too much, and their costs are rising dramatically every year. The diagnosis is pessimistic all around. There is thus a convergence across the advanced, industrial democracies treated by these studies that health care is a serious public policy problem.

Likewise, there is another, almost more disturbing, convergence in evaluation. In these stories, almost everything has been tried—from centralization to fragmentation of health systems, from market-oriented to command-oriented mechanisms of governance. Yet there are precious few success stories. To be sure, complex nuances are given their due. Foote, for example, notes the excellence and sophistication of medical devices in this country, the best in the world. Yet she could not make clearer the dysfunctional character of government–business relations in this important subsector of the health care universe.

Hence, a good deal of pessimism about the pervasiveness and intractability of the health policy dilemma reigns among these scholars. From a public policy perspective, unfortunately, the prognosis is, therefore, not very encouraging for these systems as they reach for the Twenty-first century.

*Georgia Institute of Technology* DAVID WILSFORD

**Bureaucracy, Economy, and Leadership in China: The Institutional Origins of the Great Leap Forward.** By David Bachman. New York: Cambridge University Press, 1991. 262p. $42.50.

**Bureaucracy, Politics, and Decision Making in Post-Mao China.** Edited by Kenneth G. Lieberthal and David M. Lampton. Berkeley: University of California Press, 1992. 401p. $45.00.

Is China's policymaking tightly controlled by a few top leaders, who, in essential agreement, make all major decisions, which are then carried out by a highly centralized bureaucracy? Or are there much more complicated political processes at work? Are the leaders divided into factions? What are the roles of the bureaucracy—the central and provincial cadres? Have policymaking processes changed in post-Mao China? These questions have been of great interest to students of China and are the subjects of intensive inquiry of many writings, including these two books.

Placed in context, these two volumes represent a new wave of studies on China's policymaking and products of post-Mao scholarship, and are thus different from earlier publications. In the 1970s, in the wake of Mao's Cultural Revolution, the first wave of China scholars devoted themselves to study in the politics of policymaking (e.g., Ahn's *Chinese Politics and the Cultural Revolution* [1976] and MacFarquhar's *Origins of the Cultural Revolution* [1974–83]). They were dissatisfied with the totalitarian model that had dominated the study of Chinese politics since 1949, taking issue with the notion of "Mao in command," an assumption explicit or implicit in many writings on China.

Policymaking in China involved a complicated conflict and consensus-building process with many actors and many problems that (although distinctive in many respects) had some similarities to political processes elsewhere. Although Mao played an extremely important role in the system (different from the less important role he played after the early 1960s), he was frequently blocked and frustrated by other leaders and was not always "in command," as some scholars have maintained. When Mao maneuvered on various occasions to push his policies, he sought "outside" support to overcome his opponents at the center. Consequently, the arena of political conflict was expanded; and more actors were drawn into participation in the resolution of conflict. Despite Mao's enormous power, policy was significantly influenced by debates and conflict among the leaders. Finally, leaders and cadres at the provincial and lower levels did have an effect on policy during the implementation phase in a variety of ways, although they may not have directly participated in its formulation.

Such an approach, called by some the *elite-oriented* model of policymaking, by others the *power* model, differs from the *rationality* model in the literature, which tends to stress the ought-to behavior of decision makers, who identify the problems, rationally examine the alternative means, and select the most efficient and economic solutions. Rather, the power model focuses on the leadership conflict and struggle at the top that produced major policy shifts in the agricultural collectivization, Great Leap Forward, and Cultural Revolution.

In *Bureaucracy, Economy, and Leadership in China,* David Bachman presents a provocative reinterpretation that highlights the role of bureaucracy. In contrast to standing accounts that the Great Leap Forward was the result of Chairman Mao's initiative (constraints upon Mao by others notwithstanding), Bachman sees the "institutional origins" of the Maoist policies and emphasizes an institutional process of policymaking. This process involved three coalitions of bureaucratic organizations (the financial coalition, the planning and heavy industry coalition, and the party), whose interactions structured the leadership's range of policy choices.

*Bureaucracy, Politics, and Decision Making in Post-Mao China* brings together a dozen research papers and provides detailed analyses of various segments of China's huge bureaucratic structure, with virtually every chapter informed by interviews and field research, as well as documentary sources. This "new wave" of young China scholars, like Bachman, are constructing a "bureaucratic model" in the study of Chinese politics. What distinguishes their approach is that they focus mainly on the role and the power of the great bureaucracies in the making of policy.

In chapter 1, "Fragmented Authoritarianism," one of the coeditors, Kenneth Lieberthal, argues that authority below the very pinnacle of the Chinese political system—and consequently the increased bargaining in the bureaucracy—is fragmented and disjointed. In chapter 2, "A Plum for a Peach," David Lampton analyzes the framework and the conditions that structure bargaining behavior in the bureaucratic system. The implications are, thus, that decisions are generally slow; that the process of consensus building and negotiation is protracted; and that decisions are often made "in principle" but the implementation is subject to negotiation. Chapter 3, "The Chinese Political System and the Political Strategy of Economic Reform," by Susan Shirk, applies the bureaucratic politics model to China's post-Mao reform and evaluates reform strategy and its implementation in the context of give-and-take.

Part 2 consists of two chapters, which focus on the center. Carol Lee Hamrin's "The Party Leadership System" (chapter 4) is a detailed analysis of the evolution, structure, function, and politics of the "leading group" headed by the top leaders. Chapter 5, "Information Flows and Policy Coordination in the Chinese Bureaucracy," by Nina Halpern, explores the development of "think tanks" at the center and their roles and impact on the policy process.

In part 3, four writers take up various bureaucratic clusters: Jonathan Pollack, the military (chapter 6); Lynn Paine, the educational system (chapter 7); Melanie Manion, the personnel system (chapter 8); and Barry Naughton, government and enterprise in the reform process (chapter 9). Finally, in part 4, three authors focus specifically on subnational levels of the system: Paul Schroeder on the province (chapter 10), Andrew Walder on the municipalities (chapter 11), and David Zweig on the countries and townships (chapter 12).

Taken together, these seven chapters present a complicated process of conflict resolution and consensus building at work in China. The composite picture is vastly different from the one seen from the eyes of Mao or Deng Xiaoping (see Salisbury's *The New Emperors* [1992]). The "emperor" may set the policy, but the bureaucracy below often puts up countermeasures; thus, whereas the pressure from above is ruthless and unremitting, evasion from below is resourceful and not unavailing.

China's reform and open-door policy in the 1980s have facilitated research on China in a number of ways. In general, more information has been published or become available, including the memoirs and the hitherto classified speeches of the leaders. Western scholars now have the opportunity to do field research in China and conduct interviews with Chinese officials, including the decision makers themselves. Consequently, scholarship on China has been advanced. One major contribution of the writers of these two volumes is to add to the elite-oriented approach the bureaucratic politics model of Chinese system. (See Lieberthal and Oksenberg's pioneering study, *Policy-making in China* [1988].) Their writings highlight China's structure of bureaucratic authority and the realities of bureaucratic practice that affect both the elite and the basic building blocks of the system.

The limitations of the bureaucratic politics model must also be noted. Bureaucratic politics does not explain the 1989 crisis of Tiananmen, nor does it explicate the revival of the reform and open-door policy at the Fourteenth Party Congress in October 1992; that is to say, politics in China is more than bureaucratic politics, and there are political processes other than bargaining at work.

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**Citizenship and Nationhood in France and Germany.** By Rogers Brubaker. Cambridge: Harvard University Press, 1992. 270p. $35.00.

**Ethnicity and Nationalism in Post-Imperial Britain.** By Harry Goulbourne. New York: Cambridge University Press, 1991. 271p. $54.95.

**The Frontiers of Citizenship.** Edited by Ursula Vogel and Michael Moran. New York: St Martin's, 1991. 210p. $49.95.

During the 1980s, political science and sociology discovered citizenship. Yet the seminal work to which much of the current literature refers first appeared over 40 years ago, with Marshall's *Citizenship and Social Class* (1950); and other notable contributions, influenced by Marshall, such as Bendix's, *Nation-Building and Citizenship* (1964) and Parson's "Full Citizenship for the Negro American" (*Daedalus,* [1965]) are not recent. Indeed, citizenship and writing about it goes back to Greek antiquity.

During the 1980s, the very reference points of political and social action could no longer be taken for granted. In these circumstances, the question "Where am I?" readily transforms into "Who am I?" Because it refers to membership in political community, the concept of citizenship is particularly efficacious in making sense of political identity. Thus, uncertainty about the latter raises questions of citizenship.

Membership is not a simple notion, a fact reflected in the quite different approaches to citizenship in the books under review. Membership implies nonmembership.

The salient political community in the modern world is the nation–state. Rules of *access* to citizenship status separate citizens from noncitizens. The legal possibilities of such rules include *jus sanguinis* (citizenship by descent) and *jus soli* (citizenship by birthplace). The question of access to citizenship status becomes crucial when the internationalization of economic activity and the transformation of political units move significant populations across national boundaries. Brubaker and Goulbourne focus on this aspect of citizenship.

In addition to access, there is a second axis of citizenship, which relates to what it is that membership, once attained, provides. This is its *quality*, what rights and duties attach to citizenship. Marshall distinguished between the civil, political, and social components of citizenship as sets of rights derived from judicial, legislative, and social services institutions, respectively. These institutions have independent histories; and the quality of citizenship will therefore vary between nation–states and within the same nation–state at different times, depending on local circumstances. Most of the contributions to the book edited by Vogel and Moran deal with questions of the quality of citizenship.

Feminist critiques of modern citizenship have succeeded in bringing together the different issues of access and quality. In *The Frontiers of Citizenship*, coeditor Ursula Vogel asks, "Is citizenship gender-specific?" Within a nation–state, access to citizenship status is potentially problematic for outsiders, foreigners, and politically marginal groups (separated from the majority by religion, language, or pigmentation). Yet Vogel shows that a central institution of Western society, marriage, has historically excluded exactly half of those subject to it from modern citizenship.

Vogel shows not only that when they crossed the threshold of marriage, women ceased to be members of the state in their own right but that this subordination was an integral part of men's citizenship. This situation is well documented in the chapter; but in focusing almost exclusively on the sexual dialectic, the possibility of an explanation in a larger framework is lost. Vogel does make the point, however, that before the end of the eighteenth century, marriage was not "a sentimental bond concluded for the sake of individual happiness" but a means "to guarantee the order of civil society by producing legitimate heirs for the transmission of property" (p. 73). An obvious question is, What happens to female citizenship when marriage does become merely a sentimental bond and the property question is not at issue? Vogel does not pose this question; but her likely answer might be inferred from her remark that today "a women's rights of social citizenship are, in many respects, still defined by her relation to others" (p. 77).

Social rights have a general salience because the welfare component is a distinctive feature of late modern states. Desmond King's chapter in the edited volume, "Citizenship as Obligation in the United States" shows how the New Right critique of welfarism (especially the programs of the 1960s) contributed to the Reagan administration's Family Support Act of 1988. King indicates that the earlier 1935 Social Security Act, while establishing clear social rights for the individual, empowered the states to enact their own welfare measures and administrative arrangements. This resulted in significant differences between states. A further divergence between social right and welfare policy was introduced in the 1988 act through the mandatory work or training requirement for those in receipt of welfare. Thus, federalism and punitive obligations have tended to make a serious inroad on American social rights.

Yet Michael Moran argues that economic decline has not led to a significant decline in health care entitlements. In his contribution, "The Frontiers of Social Citizenship," Moran demonstrates that health care citizenship expanded, rather than contracted, in all Organization for Economic Cooperation and Development countries, including the United States, after the onset of the world recession in 1973. His explanation is that health care entitlements "command the intense support of popular majorities" and the "burden of 'cost containment' fell on providers" (pp. 49, 51). While this might be good for the present, it does not forebode a promising future. Moran mentions that (1) demographic developments suggest that in the future health care entitlements will be an object of intergenerational political struggle and (2) it will be increasingly difficult to contain costs at the expense of providers.

These studies show that because social rights depend on budgetary considerations, they are the most vulnerable and therefore precarious of citizenship rights. But it is not only the quality of citizenship that is subject to politically determined change. The settlement of large migrant populations in European nation–states raises highly contentious issues of citizenship in many of them.

In the contemporary world, France and Germany bear close comparison. Their migrant populations, in particular, grew extensively from the 1960s and today form the basis in each country of a second generation of foreign settlers. While France and Germany have been subject to similar migration processes, have comparable immigrant populations, and converging immigration policies, naturalization rates in France are much higher than in Germany. In *Citizenship and Nationhood in France and Germany*, Brubaker explains such differences in the civic incorporation of immigrants in terms of the quite different conceptions of citizenship that have emerged in France and Germany. Indeed, his achievement is to trace the development of citizenship as the basis of membership in the French and German nation–states. In doing so Brubaker illuminates the processes of nation–state formation, as well as accounting for the different practices and politics of immigrant incorporation in those societies.

Brubaker's stated intention is to move away from the treatment of the state as a territorial organization and focus on the neglected view of it as a membership organization, as an association of citizens. Citizenship as a body of rights is, therefore, less important to Brubaker than citizenship as an ascriptive form of closure. This is an interesting perspective. It leads Brubaker to hold that as a formally defined and externally bounded membership status, citizenship was the product not of the internal development of the modern state but of interstate relations within a loosely integrated state system (p. 70). In its bold form, this perspective cannot be sustained, as Brubaker's own narrative indicates a number of times. This cannot be an alternative to the conventional viewpoint, but it does complement it in significant ways.

The old saw that France is home to civilization (rational, universal, inclusive, and expansive), Germany, to culture (spiritual, particular, exclusive, and local) is shown by Brubaker truly to reflect the different traditions of nationhood in each country. The French nation–state was the product of centuries of state building. National consciousness developed gradually within a territorial state and its socializing institutions of schoolroom and barracks. The German nation–state, on the other hand, came from more mixed traditions, which included a national consciousness formed outside and against the territorial and institutional frame of the pre-Bismarckian German states. It is in these formations of the nation–state that quite different types of citizenship emerged. French citizenship is attributed to a child born in France if at least one parent was also born in France (including French colonies and territories prior to independence). German citizenship, on the other hand, is based on ethnocultural descent; and birth in its territory has no bearing on the matter, even for second- and third-generation settlers.

Brubaker develops this position in a historical, institutional, and cultural argument that takes us up to the present day politics of citizenship. This is an important book and provides a striking corrective to an exclusively rights-based account of citizenship. But Brubaker's historical perspective on the development of nation–states introduces its own narrowness—which finds its correction in Zig Layton-Henry's "Citizenship and Migrant Workers in Western Europe" and Elizabeth Meehan's "European Citizenship and Social Policies" in the edited volume. The simple truth is that the structure of citizenship is changing. If we move away from the definition of citizenship as membership in a nation–state and toward one of membership of a community of legal rights and obligations, we arrive at a picture both more complex and richer than the one Brubaker leaves us with. Layton-Henry shows that in Western Europe today there are different levels of citizenship participation insofar as nonnational residents may have civil and social rights and even certain political rights by virtue of the laws of their host counties. Meehan shows that the transnational European Court of Justice guarantees social rights to workers and their families within the European Community irrespective of their immigrant status.

Whereas French and German citizenship has remained more or less unchanged in recent times, British citizenship has been significantly modified since the loss of Britain's colonies from the late 1940s. This is the subject of Paul Rich's "Patriotism and the Idea of Citizenship in Postwar British Politics" in the edited volume and also Harry Goulbourne's *Ethnicity and Nationalism in Post-Imperial Britain.* Rich provides a succinct and highly readable account of the politics around and between the 1948 Nationality Act, which appeared to provide free rights of entry to colonial immigrants, and the 1981 Nationality Act, which offered no such illusions. Goulbourne covers the same ground and more. Part of his argument is that immigrant decisions to take up British nationality or not are partly shaped by the politics of the nation–states they leave (a point also developed by Brubaker). He therefore includes two chapters on diasporic politics, in addition to discussing the changes and prospects of citizenship and nationality in a postimperial and multiracial Britain. The discussion is not only scholarly but at times programmatic and is always informative.

Each of these books indicates the liveliness of citizenship in current politics and scholarship. As Vogel and Moran note in their introduction, citizenship has arguably overtaken class, market, and even democracy as the "strategic concept of political science" (p. x). Each of the books under review gives some clear indication of why this is so.

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**The Two Churches: Catholicism and Capitalism in the World System.** By Michael L. Budde. Durham: Duke University Press, 1992. 172p. $29.95.

**Popular Voices in Latin American Catholicism.** By Daniel H. Levine. Princeton: Princeton University Press, 1992. 403p. $49.50 cloth, $18.95 paper.

**The Church and Politics in the Chilean Countryside.** By Hannah W. Stewart-Gambino. Boulder: Westview Press, 1992. 200p. $49.95.

Rethinking longstanding traditions of interpretation within mainstream social science, many recent efforts to understand the place of religion in the process of social and political development stress its role as an independent variable. Michael L. Budde, Daniel H. Levine, and Hannah W. Stewart-Gambino make it a point to distance themselves from reductionist views of religion, preferring instead to emphasize the complex and unpredictable character of religious phenomena and the potential autonomy that churches as strategic actors can enjoy from surrounding social and cultural milieu. Indeed, each of these authors explores the notion that the Catholic Church in Third World contexts—Latin America providing the primary *situs*—can play a major role in advancing the interests of the poor and the marginalized. However, though all three of these works seek to analyze the role of the Catholic Church in the process of development, each does so from a very different perspective.

Hannah Stewart-Gambino focuses her study of the Catholic Church on a single country, Chile, and on a single issue dimension: the Church's rural policy in twentieth century. The author poses a puzzle at the outset of her work which forms the core problem she addresses: Why did the multiple Church efforts to organize the Chilean working class before the 1960s ignore the peasantry? And, relatedly, what best accounts for the massive Church-sponsored efforts to organize the peasantry beginning in the 1960s? Stewart-Gambino suggests that the Church's changing social policy is not fully understood by changing doctrine or beliefs. Rather, she argues that the relatively weak religious cleavage that characterized twentieth century Chilean politics left the Church in a weak and uncertain political position vis-a-vis other social and political actors (such as its perennial foe, the Marxist left), and drove the Church into the arms of the Conservative party, the stronghold of landholding elites. The Church had to pay a price for its alliance with the Conservatives: continued cooperation with landed oligarchs on the agrarian question. Not until the Church found a politically viable alternative to the Conservatives in the late 1950s in the reformist Christian Democrats did the Church dare to involve itself with the socially explosive issue of peasant mobilization. A key to Stewart-Gambino's explanation for changing Church rural policy is that, in the absence of a strong religious cleavage and the concomitant enduring political loyalties, the Church was too weak to act on its own and was forced to exercise its influence through the Conservative party.

Stewart-Gambino's focus on changing patterns of political and social mobilization in the countryside represents a most welcome shift in the literature on twentieth century Chilean politics. For too long the field has underestimated the political importance of the rural sector, particularly egregious in Chile given the salience of the agrarian question for the political system as a whole, and the Catholic Church in particular, during the 1950s, 60s, and 70s. One of the chief contributions of this book is, therefore, its account of the bitter struggle among Church leaders over shifting strategic political alliances, and the related fundamental changes in Church attitude concerning its socio-political role in the countryside. The eventual explosion of Church-sponsored institutional initiatives in the countryside in the late 1950s and 60s, and its reformist alliance with the Christian Democratic party, was the outcome of decades of battles between progressives and traditionalists within the Church.

Stewart-Gambino is correct in posing the absence of Church social programs in the countryside as an anomaly. Indeed, she perhaps does not go far enough. The Catholic Church was not alone in declaring the rural sector off-limits; centerist and leftist parties had acquiesced in the exclusion of the peasantry from social legislation during the entire period of the popular front. As numerous works have shown (see especially the Colliers [1991]), the social and political incorporation of labor was almost entirely an urban phenomenon in Chile (as was the case in Brazil as well) until the 1960s. The puzzle posed by Stewart-Gambino, therefore, might very well be cast more broadly: Why was the peasantry in Chile systematically excluded from the gains achieved by urban labor in the first half of the twentieth century? The explanation, as Stewart-Gambino hints, rests somewhere with the relative strength of the Chilean oligarchy and, until the late 1950s, the ongoing electoral successes of its principal political vehicle, the Conservative party.

The author's attempt to explain changing Church social policy in the Chilean countryside in terms of a "relatively" weak religious cleavage and a correspondingly weak institutional identity runs into considerable trouble. One is left to wonder, "A weak cleavage relative to what?" The only comparison Stewart-Gambino brings to support her claim is the case of Colombia, notorious for its violent religious battles, its powerful Church, and its traditional two party system. Yet, because the political legacy of Chile's religious cleavage may not be as strong or clear-cut as the most notorious case in all Latin America, it does not necessarily follow that the cleavage in Chile was "weak." Indeed, given the critical importance of the clerical-anticlerical cleavage in shaping Chilean politics over time, it would be difficult to find a stronger case within Latin America, with the single obvious exception of Colombia. By contrast to Stewart-Gambino's thesis, there exists good reason to believe that Chile's religious cleavage, and more importantly, the way that cleavage became politicized in the nineteenth century, endowed the political system with a strong and enduring religious cleavage well into the twentieth century.

From the inception of competitive party politics in mid-nineteenth century Chile, the Church played a major role in structuring the party arena. Unfortunately, Stewart-Gambino's account of "The Church and the origins of the Chilean party system," the title of her second chapter, fails to make mention of the intimate role played by the Church (and, specifically, that of Santiago's Archbishop Valentin Valdivieso) in the very creation of the clerical Conservative party in 1857. There is widespread agreement in the literature that the clerical-anticlerical conflict was the defining political cleavage of the nineteenth century, with all major parties deriving their identities along this continuum. In fact, Stewart-Gambino's assertion that "the battles over clerical issues [in the nineteenth century] did not result in a religious cleavage that endured into the twentieth century as it did in many countries" (p. 15) is contradicted by the interesting narrative of her book. As she correctly notes, for the first half of the twentieth century, the clerical Conservatives continued to perceive themselves, and be perceived, as the party of the Catholic Church and to enjoy the electoral and ideological advantages that came with the turf. While it is true, as the author argues, that class became the predominant cleavage by the second decade of the twentieth century in Chile, it would be false to conclude from this that the religious cleavage, and longstanding political identities and subcultures, simply evaporated. Rather, the political landscape in post-1930 Chile should be viewed in terms of two overlapping cleavage dimensions, with class cross-cutting religion and redefining (but not erasing) the earlier political arena. The initial crystallization of the party system in Chile in the mid-nineteenth century left identifiable party legacies (for example within the clerical Conservatives and anticlerical Radicals) that endured well into the twentieth.

The author's assertion that, after official disestablishment in 1925, the Catholic Church "was relegated to the private sphere of Chileans' lives" (p. 20) is again belied by the storyline followed by the rest of the book. As the author quite ably shows, the Catholic Church pursued its interests very aggressively through the Conservative party, and after the 1950s, via its new political ally, the Christian Democrats. Is this weak and defensive Church the same one that forced an anticlerical Radical party president, Aguirre Cerda, to make concessions or face political gridlock? Is the intense conflict between Catholic progressives and traditionalists during the 1930s, 40s, 50s, and 60s, a conflict culminating in a dramatic electoral face-off between two competing Catholic parties, best viewed in terms of an inert religious cleavage? Is the massive mobilizational effort in the countryside by Catholic activists, and the unprecedented electoral margin achieved by the candidacy of Eduardo Frei in 1964, consistent with a relatively weak institutional Church? Finally, how does a Church suffering from a "relatively low degree of national influence or power" (p. 163) at the same time develop a network of local and national organizations to create a protective "umbrella" (p. 154) to consistently oppose the Pinochet dictatorship? All of these questions point to a more salient religious cleavage and a stronger institutional Church than Stewart-Gambino allows.

Michael L. Budde complains that recent studies of Catholicism, like those of Stewart-Gambino and Levine, have been limited to nation-based studies (p. 4). He argues that what is missing in the analysis of contemporary Catholicism is an appreciation of the place of Catholicism as a transnational actor within the world capitalist system. Embracing Wallerstein's world systems theory, with a dab of dependence literature, Budde posits "the existence of a single world economy, capitalist in nature, originating in the sixteenth century and by the current age encompassing all areas of the world" (p. 4). Budde sets out to explore the Catholic Church in the "periphery" and the "core." Unlike Stewart-Gambino or Levine, Budde makes no efforts to "produce a positivistic core of falsifiable or quantifiable generalizations" (p. 3). Rather than original empirical work, Budde offers "transdisciplinary wanderings, covering a broad expanse of literature and research" (p. 3).

At the start of his analysis, he notes a seemingly incontrovertible fact: the Catholic Church is experiencing an historic global shift from a First World to an overwhelmingly Third World Church within the next decade. After analyzing the major trends within the Catholic Church in both the core and the periphery, he draws two major conclusions. First, "the workings of the world capitalist economy and changes within the Catholic Church make it plausible to argue that world Catholicism (led by the Latin American and other Third World Churches) will continue to develop in anticapitalist directions" (p. 3). This is necessarily so because of a multitude of structural and non-structural factors pushing Third World Catholicism in an anticapitalist direction. Inexorably increasing "proletarianization, marginalization, and emmiseration" (p. 40) on the periphery, combined with the global proliferation of highly effective, participatory, and ecclesially "tight" basic Christian communities (CEBs), will eventually lead to increasing solidarity of the Third World Church in opposition to the Church at the core. Second, "the Churches in core countries (typified by the U.S. Catholic Church) are poorly placed to respond in ecclesial solidarity to the anticapitalist critiques and challenges of their Third World coreligionists, who now compose the majority of members worldwide in the Catholic Church" (p. 3). The Church of the First World, beneficiary of the systemic injustices of the workings of world capitalism, and hamstrung by a "loose" and undemanding ecclesiology, is genetically indisposed to confront the hard truth. Budde argues that the future of worldwide Catholicism holds a colossal clash between the two Churches, pitting poor against rich.

Though certainly provocative, Budde's "transdisciplinary wanderings" leave a number of unsettling questions. One wonders about the theoretical justification for focusing on Latin American Catholicism as "archetypal of peripheral-semiperipheral Catholicism in matters of political economy and ecclesiology" (p. 13). Familiarity with the fastest growing Catholic Church in the periphery, the relatively conservative Church in sub-Saharan Africa, or the Church in Southeast Asia (such as the Church in Bangladesh), suggests that Budde's "archetype" is not especially useful for understanding Catholicism in all peripheral contexts. As Daniel Levine's work emphasizes time and again, it is an exaggeration—even in the most "liberationist" Churches in Latin America such as Brazil or Salvador—to suggest that basic Christian communities and the option for the poor have become widespread pastoral practice throughout Third World Catholicism (p. 60). Membership in these "tight" ecclesial practices is almost always miniscule compared to those who remain within more traditional parish structures. Furthermore, again as Levine painstakingly documents, it is naive to state that "almost all CEBs emphasize egalitarian and democratic internal values and seek to apply the Gospel to their everyday reality" (p. 54). After reading Levine's detailed study, one suspects that there are as many differences between national ecclesial approaches within the Third World (even between Latin American neighbors like Colombia and Venezuela) than there are between Churches of the First and the Third Worlds. For example, to simply assert that "the [Latin American] Church found itself in the unusual position of opposing the military" (p. 13) during recent authoritarian experiences certainly does not describe accurately the behavior of the contemporary Argentine Church.

Just as the Latin American Church serves as an archetype for peripheral Catholicism, Budde asserts that the U.S. Catholic Church is an appropriate archetype for Catholicism in the core. After reviewing the ecclesiological assumptions of five U.S. Catholic moral theologians, Budde arrives at the conclusion that the Catholic Church in the United States, and by extension, the entire First World Church, cannot possibly become critical enough of capitalism to maintain religious solidarity with the Third World Church. It is simply too much to ask that mainstream theologians in rich countries come to see the systematic exploitation brought upon the world's poor by their own country. Therefore, "the most likely scenario is serious class conflict within world Catholicism" (p. 133) between the opponents of capitalism in the Third World Church, and its uncritical supporters in the core. Despite his insistence that "religious phenomena retain at least some spark of independence from their surrounding milieus" (p. 2), it seems that, in the end, the First and Third World Churches are incapable of overcoming their social and economic milieu.

Daniel H. Levine's work in some ways represents a methodological middle ground between the case study approach used by Stewart-Gambino and the more global analysis employed by Budde. Levine is very self-conscious about building into his study variations of many kinds, both between and within nations, institutions, groups, and categories of individuals such as peasants and city dwellers, men and women, laity and clergy. As Levine points out, "this comparative and multilevel structure allows for a more thorough exploration of the origins and pathways of change than would be possible with a focus on any single case or dimension of the process" (p. 25). Drawing on a vast store of primary and secondary data collected during three years of field work in peasant villages and urban neighborhoods in Colombia and Venezuela, Levine's study is an artfully crafted study of "popular voices in Latin American Catholicism." By contrast to the approach taken by most studies on the Church, including those of Stewart-Gambino and Budde, Levine adopts a phenomenological perspective, allowing the poor to speak for themselves. "Ordinary people have a central role to play, and their ideas are as vital to analysis and understanding as are the more easily documented positions of elites and formal institutions" (p. 29). The reader thus is invited into the lives of ordinary people as they create groups, find leaders, and take initiatives, often apart from the Church hierarchy.

Levine's book is an extremely ambitious attempt to study religious change and cultural transformation in order to understand and explain changes in the religious expression of popular groups, changes in culture, and the potential impact these changes might have on associational life. He grounds the analysis of cultural change in evolving "linkages" between ideas, group structure and practice, class, and most especially, institutions. Institutional configurations become especially important for his analysis because "popular voices find expression in contexts shaped by institutions and their agents, and by the needs and understandings (derived from class, economic circumstance, gender, politics, and community tradition) that popular individuals and groups bring to their encounter with institutions" (p. 28). His analysis suggests that new forms of religious experience may eventually be related to "the development of a new vocabulary and structural basis for independent moral judgement and group solidarity" (p. 12).

Levine's analysis begins by carefully placing each of the two national Churches he focuses on, Colombia and Venezuela, in their respective social, political, economic and ecclesial contexts. He draws a relatively sharp contrast between the two cases, noting that, in Colombia, "hierarchy is the order of the day, with little legitimate place for popular mobilization or for the construction of democratic patterns within organizations" (p. 91). By contrast, political norms that value equality and participation over hierarchy and subordination have given both activism and independent organization a legitimate place in Venezuelan national culture. Levine argues with great subtlety that the predominant ideas of any culture become linked to structures and become the underpinnings of what is taken as routine, "the stuff of common sense" (p. 92). If religion is to play a role in the enduring transformation of popular culture, then religion itself must change and become incorporated into the daily expectations that ordinary people bring to bear on decisions about legitimate priorities, commitments, and action (p. 133).

After mapping the two national churches at the level of structure and organization, Levine moves to a lower level of analysis, tracing the "origins, pathways, and meanings of change through the eyes and with the worlds of those involved" (p. 129). Reminiscent of de Tocqueville, Levine interviews dozens of priests, sisters, pastoral agents and community members in 19 different neighborhoods and observes their pastoral interactions in order to uncover the construction of discourses and ways of living together that provide the foundations for views on judging right and wrong and the standards for weighing legitimate and effective leadership. In the smallest details of articulated attitudes and beliefs that Levine seeks to uncover the potential roots of a transformation of popular images of self and community, and the attendant reworking of views on basic issues like activism, passivity, hierarchy, and equality (p. 179).

Throughout his analysis, Levine avoids euphoric claims about the possibilities of transforming associational life through the experience of basic Christian communities. He wisely points out that popular groups tend to be consistently less radical and much less prone to utopian expectations than those who write about them (p. 365). But still, there is something genuinely new occurring in Latin American Catholicism, represented by liberation theology and the spread of basic Christian communities. Where popular culture is nurtured in these base communities, sustained cooperative action is more possible, and association becomes legitimate in religious terms. The development of a strong associational life can provide the underpinnings for a truly independent civil society. And "the resulting shift of popular culture from resignation, fatalism, and silent powerlessness to equality, activism, organization, and voice is a cultural and political change of major proportions" (p. 351).

*University of Notre Dame* TIMOTHY R. SCULLY, C.S.C.

**The Radiant Past: Ideology and Reality in Hungary's Road to Capitalism.** By Michael Burawoy and János Lukács. Chicago: University of Chicago Press, 1992. 174p. $24.95.

In the aftermath of communism's precipitous collapse in the Soviet Union and Eastern Europe, social scientists are rethinking their conceptions of state socialism and trying to construct theories to assess the region's political and economic prospects. *The Radiant Past* is a unique and valuable contribution to this burgeoning literature. Coauthored by two sociologists, an American and a Hungarian, the book is a series of theoretically sophisticated essays, written between 1983 and 1990, reflecting on the studies that Burawoy and Lukács conducted at two Hungarian factories, a machine-building plant and a steel works. Against all odds, Burawoy was able, as an American academic, to obtain permission to work as a radial drill operator for 2 months at Bánki, the machine-building division of a heavy vehicle manufacturer and then for 11 months as a furnaceman in the October Revolution Brigade of the Lenin Steel Works. At the same time, Lukács studied the management of the two enterprises. Later, as communism collapsed in Hungary, the two men's roles were reversed, with Lukács becoming one of Hungary's leading advocates of employee stock ownership plans and Burawoy taking over as the nonparticipant observer.

As ethnography, *The Radiant Past* is a fascinating and engagingly written eyewitness report on proletarian life in the waning years of goulash communism. It vividly portrays Hungary's flourishing second economy, its widespread consumerism and alcoholism, its conservative gender politics, and its more or less open cynicism about socialist elites. In its description of workers' self-organization on the shop floor and their initiatives in the second economy, it serves as an interesting foil to Miklós Haraszti's earlier and far grimmer portrait of Hungarian factory life, *A Worker in a Worker's State* (1977), which depicted an atomized and despotically controlled working class.

But while *The Radiant Past* focuses on Hungary, its theoretical framework is a comparative one, across different state-socialist societies and between state socialism and capitalism. It builds on Burawoy's 1977 book *Manufacturing Consent*, which reported on his work as a radial drill operator at Allied, an American machine factory. The Hungarian firm Bánki (closely comparable to Allied in size, technology, and institutional position as a supplier to other divisions within a single enterprise) confounded stereotypes of socialist industry by being considerably more efficient at the shop-floor level than its American counterpart. And while Burawoy found that his American coworkers at Allied lacked a sense of class consciousness, Hungarian workers at Bánki and the Lenin Steel Works shared a strong sense of their exploitation by the triple socialist elite of management, party, and union. Nevertheless, Hungarian workers were politically demobilized, unlike their Polish counterparts in the Solidarity movement.

These observations form the basis of the book's two central theoretical claims. The first uses the unexpected contrast between Allied and Bánki to criticize conventional models for comparing capitalist and state-socialist production relations. A finer-grained institutional analysis is needed, Burawoy and Lukács contend, to understand what makes capitalist and state-socialist firms run efficiently or inefficiently. They argue that a firm's efficiency depends primarily on organizational hierarchies, not on property relations. A socialist firm, faced with shortages in state-allocated resources, may respond by granting workers more autonomy to adapt to changing supplies of raw materials, making production efficient at the microlevel. By contrast, a capitalist firm cushioned from market constraints (e.g., a division, like Allied of a large enterprise) may operate with all the inefficiencies of a stereotypical bureaucratic socialist factory, especially given the environment of rigid union and management rules characteristic of some American industries. The model offered by Burawoy and Lukács expands on the "new institutional analysis" approach of Stark and Nee in *Remaking the Economic Institutions of Socialism* (1989) and on János Kornai's analysis of state socialism in *The Economics of Shortage* (1980), providing a useful framework for assessing the economic and political constraints, as well as the adaptive options that firms face in a variety of institutional contexts.

The second major claim of *The Radiant Past* is an ironic twist on Marxist theories of class consciousness. Burawoy and Lukács argue that it is state socialism, not capitalism, that generates dissent among the working class. As a system in which an elite is clearly in charge of the allocation of resources, state socialism needs to legitimate itself by invoking socialist ideology. In the factory, this is done through what the authors call "rituals of socialist affirmation," which have the effect of constantly calling attention to the gap between ideology and reality. Hence, while Burawoy had argued in his earlier book that capitalism "manufactures" consent, *The Radiant Past* argues that state socialism produces dissent. Workers in state socialism come to define themselves in relation to a common exploiter. Under certain conditions, such dissent can lead to political mobilization, though in Kádár's Hungary, haunted by memories of the 1956 Soviet invasion and with opportunities for individual advancement through the second economy, it led instead to political cynicism and individual opportunism.

This argument about the relationship between ideology and dissent in state socialism is intriguing, in part because it considers the nature of dissent among workers, a group often ignored in writings on dissent in the Soviet bloc. And while Burawoy and Lukács focus solely on workers, not on the party elites whose behavior was so crucial to the events of 1989, their argument may have wider application in helping to explain how communism collapsed so quickly.

*The Radiant Past* closes with an account of how Hungary's privatization laws have enabled leading cadres to reestablish themselves as a capitalist elite, and with a discussion of a number of cases in which workers have built on the shop-floor organization of the socialist past to challenge the new elites of the capitalist future. While Burawoy and Lukács offer a valuable and sobering corrective to the free-market utopianism of many commentators on Eastern Europe, their most daring conclusions are also their least convincing; and the book ends with sweeping claims about the apocalyptic ills of capitalism and the radiant future of the socialist project that fit oddly with the subtler arguments offered earlier. Capitalism is, indeed, unlikely to offer a radiant future to Burawoy's former comrades at the Lenin Steel Works; and the Kádár era may well take on a nostalgic glow in their memories. But will these men and women embrace either the hodge-podge of Kádárism or genuine democratic socialism as a radiant vision? After the events of 1989, it is imprudent to call anything impossible. But *The Radiant Past* does not show how these are real possibilities.

Despite my reservations about its grander conclusions, I found *The Radiant Past* to be a richly rewarding book, one that should interest political scientists in a variety of subfields, from area specialists and comparativists to political economists, as well as those interested in Marxist and post-Marxist theory.

*Princeton University* ELIZABETH KISS

**Patriarch and Folk: The Emergence of Nicaragua, 1798–1858.** By E. Bradford Burns. Cambridge: Harvard University Press, 1992. 307p. $39.95.

**Elites and Democratic Consolidation in Latin America and Southern Europe.** Edited by John Higley and Richard Gunther. New York: Cambridge University Press, 1992. 354p. $49.95 cloth, $16.95 paper.

A historian and a political scientist may draw on the same material to write their books, but each enterprise has a different purpose. The political scientist aims to derive from his data general principles of human political behavior; the historian wishes to tell a story that reflects as closely as possible the most authentic sources of information. The historian's story may, indeed, reflect general principles of human behavior; and he may point this out. But the principles are used to illuminate the facts, not the other way around.

Bradford Burns's account of Nicaragua during the early nineteenth century incidentally reveals that he is familiar with ecological determinism, class analysis, and dependency theory; but general ideas figure in his narrative as more or less decorative clichés not taken seriously and certainly not examined at any depth (e.g., "the disintegration of society," "the unification of the nation," p. 70). Although the author intends that his concepts of patriarchy and folk society should be taken seriously, a sociologist would probably make short work of them. Suffice it to say that on his own terms, Burns has written a good book, a colorful narrative that draws on an unusual set of sources (e.g., contemporary journalism, foreign travelers' accounts, diplomatic correspondence) that show how the struggle between Liberals and Conservatives merely gave respectability to what was in reality a rivalry between León and Granada that grew out of the differing commercial interests of the elites of the two cities. The interpretation given the material contains no surprises, but there is no doubt that the book succeeds on its own terms.

The 10 case studies contributed to the Higley and Gunther volume also contain narratives; but they are presented not for their own sake but to test a series of the editors' hypotheses having to do with the importance of agreement among elites for the successful consolidation of democratic régimes. It does not really matter that several of the authors reject all or part of the hypotheses propounded or object to how the problem is conceptualized or the terms defined; the imposition of a theoretical framework forces them to ask and answer significant questions that give their narratives a general significance and make them contribute to the sum total of our knowledge. The theoretical framework laid down by the editors is actually very good, although (as they see themselves) it threatens always to become tautological. It is not clear to me, for example, who is elite and who is nonelite; and the fact that their conclusion classifies some states as "consolidated democracies" and others as "consolidating," "possibly consolidating," and even "possibly de-consolidating" (p. 325) suggests that the categories involved are a mite too fluid to serve as the foundation for a lasting theoretical structure. Nevertheless, the book represents an important contribution.

Normally, one deplores the unevenness of a set of contributions on a theme. I was rather taken aback by the high quality of almost all of the contributions. The only one that clearly fails is that by Peter M. Sanchez on the Dominican Republic. Sanchez misunderstands key elements of the story he tells, seriously underestimates the role of the United States, and mishandles composite Hispanic names. The criticisms that can be made of the other chapters do not rise much above the level of nit-picking, and there is much more to praise than to blame.

Alan Knight's chapter on Mexico is a virtuoso display of insight, erudition, and sophistication, although perhaps too consciously so. Knight manages to get the extremely complex Mexican picture exactly right, though I think the editors are on solid ground to reprove his rejection of their category of "stable limited democracy" in favor of "inclusionary authoritarianism," inasmuch as the same traits are used to define the one as the other (pp. 338–39). Knight's chapter will be difficult to follow for those not familiar with the history of twentieth-century Mexico (nor do the misprints help). I learned a thing or two from it, despite having been a close student of Mexican politics for 30 years.

Maurizio Cotta has some difficulty applying the editors' schema to Italy, finding that the categories are somewhat too ambiguous, the hypothesis, too circular, to capture the nuances he finds in his careful political history. The late Charles Gillespie threads his way deftly among the factional maneuvers of Uruguayan politics, indicating clearly the importance of U.S. influence, the realism of the Communist party, and the shifts in the balance of power within the armed forces—though he did not clear up what has always puzzled me, namely, how a principled progressive politician like Wilson Ferreira Aldunate could become a leader of the Blanco party. There is unfortunately nothing new in Marcello Cavarozzi's treatment of Argentina and Chile; Argentina is apparently the way it is because, well, it has always been that way.

Henry Dietz and Tom Bruneau give solid accounts of Peruvian and Brazilian politics, respectively. Bruneau is insightful but weights the reader down with an infinitely detailed blow-by-blow account of the transition process. The clarity and straightforwardness of Lawrence Graham's piece on Portugal make this the best thing by him I have seen, though I missed a discussion of the U.S. role, particularly the interesting story of how the Central Intelligence Agency came to back the Portuguese Socialist party. John Peeler's chapter on Colombia, Venezuela, and Costa Rica is a fine analysis that makes good use of the elite perspective, pointing out how the National Front in Colombia was deliberately designed to demobilize the masses. He is particularly interesting on the elitist implications of the combination of presidential system and proportional representation.

Perhaps the best use of the model is that of Gunther himself in his chapter on Spain. His account of the process of transition to democracy is outstanding for the solidity of his judgment and the sophistication of his perceptions. He is particularly insightful on the implications of the negotiation strategies and framework adopted. I think he gets the causal relations just about right, even to the role of luck. I would only stress the attractiveness of the prospect of getting in on the unification of Europe, with all that that implied for economic growth and political opportunity, as a force concentrating people's minds. This is a very useful compendium for historians, as well as for political scientists.

*University of the Pacific* MARTIN C. NEEDLER

**Political Change in Taiwan.** Edited by Tun-jen Cheng and Stephan Haggard. Boulder: Lynne Rienner, 1992. 269p. $35.00.

If democracy is defined by competitive elections with a real possibility of changing ruling groups, then Taiwan is well on its way to becoming a democracy. In the December 1992 national legislative elections, the ruling party, the Kuomintang (KMT), polled a dismal 53% of the popular vote. Although it still may control the commanding heights of the political system, the KMT is perilously close to losing its claim to be the majority party. The collection of essays edited by Tun-jen Cheng and Stephan Haggard sets forth to explain democratization in Taiwan.

Written for a conference in 1989 and revised in early 1991, the articles in this volume do not reflect significant political events in 1991 and 1992. The book does, however, provide excellent treatment of 1986–90, a period of steady movement away from authoritarianism and toward a genuinely democratic political practice. Edwin Winckler best sums up the time: "Taiwan had liberalized authoritarianism but not yet institutionalized democracy" (p. 221).

The book is organized into 10 chapters, with a short forward by Samuel Huntington. Cheng and Haggard open with an overview of Taiwan's political history intertwined with a discussion of theories of democratization. They also place Taiwan's transition from authoritarianism in comparative perspective, contrasting it with Turkey, Mexico, Japan, and the East Asian newly industrializing countries. Hung-mao Tien follows with a general description of political changes from 1986 to early 1991, reiterating his thesis (drawn out in his book, *The Great Transition*) that Taiwan is moving toward a "dominant-party system" reminiscent of Japan. In chapter 3, Hsin-huang Michael Hsiao, a leading Taiwan sociologist, surveys a number of social movements that emerged in the 1980s as instances of a "demanding civil society" interacting with a "soft authoritarian state." The evolution of the KMT from an "authoritarian and exclusionary" party to a more "pragmatic and inclusionary" organization is discussed by Taiwan political scientists Ping-lung Jiang and Wen-cheng Wu. They argue that this experience confirms earlier theories of political change articulated by Huntington and Sartori.

Constance Squires Meany also discusses the role of the KMT but does so in the analytic categories and language of the more recent theorizing about transitions from authoritarianism of O'Donnell, Schmitter, and Whitehead, among others. Her carefully crafted chapter is somewhat pessimistic, drawing attention to the limits of democratization and the likelihood of a "hybrid regime," neither fully democratic nor authoritarian, that may "persist for a protracted period." Alexander Ya-li Lu provides a very good overview of the leading opposition party, the Democratic Progressive party, echoing Tien's suggestion that Japan is the image of Taiwan's political future. Voting behavior is the focus of Fei-lung Lui's contribution, chock-full of empirical data. Fu Hu and Yun-han Chu also analyze elections. Their multidimensional log-linear analysis yields new insights into the political orientations of Fujianese and Hakka voters. Andrew J. Nathan considers the effect of Taiwan's democratization on its relations with mainland China, arguing that Beijing's terms for reunification are ill suited to the changing political realities on the other side of the Strait. Edwin A. Winckler concludes the volume with a stimulating essay that restates his oft-cited theme of the transition from "hard" to "soft authoritarianism" but also tweaks the conventional wisdom on Taiwan politics in myriad ways, from the Leninist nature of the Party–state to the role of the military in politics.

As this summary demonstrates, the book is eclectic, both in its argumentation and its specific topics. The cumulative effect, however, is salutary. Instead of the artificial discipline of a singular theoretical framework, the authors explore a number of questions and consider various interpretations. The diffuse analytic pointilism yields a nuanced portrait of contemporary Taiwan politics.

Indeed, the volume would have benefited from even greater interpretive variety. The range of theoretical frameworks runs from modernization theory to the transitions from authoritarianism literature. Though some authors are fairly creative within these confines, intriguing questions are raised but not pursued. Chief among these is the issue of Taiwan's political identity, which is very much a part of its contemporary politics and future prospects. Winckler has something to say on this topic, but there is much more to be done along these lines. If democracy is about a particular type of political identity, or identities, then Taiwan's democratization has yet to be fully explored.

*Williams College* GEORGE T. CRANE

**Conflict and Competition: The Latin American Church in a Changing Environment.** Edited by Edward L. Cleary and Hannah Stewart-Gambino. Boulder: Lynne Rienner, 1992. 233p. $36.50 cloth, $16.95 paper.

The edited-volume format, with contributions assessing circumstances in many countries, has become a mainstay in studies of the Latin American Church and politics. In this field, the multiple-contributor form serves to extend the ongoing discussion. A major new volume appears every few years as an update on developments across the region or to assess important themes (e.g., popular religion, the progressive Church).

The present volume is the most recent entry of this sort. It is a useful and timely book, one well suited for advanced students and scholars. It carries forward the ongoing assessment of Christianity and political–economic matters through the late 1980s and early 1990s.

The central themes that the authors track in diverse national circumstances are the phenomenon of religious competition (primarily Catholicism vs. Pentecostalism); the impact of the Vatican's intraecclesial retrenchment; and the changed socio–political environment of the region (newly returned "democracies," neoliberal economic policies, reconstruction of political parties, interest groups, and the like). While not a unanimous position, most of these contributions would agree with Stewart-Gambino's introductory assessment that these new challenges will probably limit Latin American Catholicism's "ability to play such a prominent and progressive role" in the region (p. 1). The limits and reversals of progressive Catholicism (typified by support for liberation theology and base communities) attract much attention throughout this volume.

In addition to the useful introductory and concluding chapters (the former written by Stewart-Gambino, the latter by Cleary), the book features eight country-specific chapters (two on Brazil and one each on Chile, Peru, Venezuela, Nicaragua, Cuba, and Guatemala). None are second-rate, while a few, especially Jeffrey Klaiber's contribution on Peru, are outstanding.

In their chapter on Brazil, Thomas Bruneau and W. E. Hewitt continue to sound the demise of progressive Catholicism in the country most identified with that theological, ecclesial, and social movement. They argue that the Base Community movement, far from being a viable, long-term form for religious and social transformation, is, in fact, in stagnation and looks more like just another in a long line of Church-generated efforts to connect with its social environment (p. 58).

Carol Ann Drogus, also writing about the base communities in Brazil, adds important insights into the role of women in those community groups and in other grass-roots movements. She argues convincingly that religious themes of liberation, ministry, and service to others are appropriated by women in distinctive and diverse ways. While her distinction between social-movement participation and electoral–activist participation as one of less political versus more political is open to question, to her credit, she notes that not all Church actors accept the identification of politics as electoral–party activity (p. 66). That Church actors do not impinge on party politics in significant ways is not, in itself, conclusive evidence that religious actors have little political influence, once one broadens the definition of "political."

Klaiber's contribution on Peru points to the persistence and structural bases of progressive Catholicism in that country and argues that the tendency is not likely to fade away in the foreseeable future (the Vatican and other pressures notwithstanding). His discussion of factors mitigating the effectiveness of the centralizing movement within the Church is careful and not overstated. Klaiber's evaluation of the Church's role vis-à-vis Sendero Luminoso and the Peruvian military is valuable and benefits from his quarter-century of living and working in that country.

The chapters by Froehle on Venezuela and Cleary on Guatemala bring fresh life to the somewhat sterile debates on Catholics versus Pentecostals in Latin America.

Froehle discusses the impact each tendency has had on the other and highlights how Catholic groups, with creative innovations in the areas of youth ministry, catechesis, and community building, have had considerable success meeting the Pentecostal challenge on the latter's own turf. Cleary, examining the Protestant upsurge in Guatemala dispels the notion of a monolithic Pentecostal movement and simultaneously calls into question some versions of secularization theory. He suggests that Christianity, both Protestant and Catholic, is experiencing an upsurge in Guatemala as traditional Indian religions collapse in the wake of the political, economic, and cultural upheavals in Guatemalan life.

While all the contributions to this volume are worthwhile additions to the literature, a few general reservations must be recorded. The first concerns the level of analysis assumed as primary in these country-by-country volumes, the present one included. Transnational factors—except for the Vatican—tend to be underplayed or ignored altogether in favor of nation-specific histories, personalities, and events. Yet many of the fundamental problems and limitations affecting all the churches of Latin America are themselves rooted in transnational phenomena (the debt crisis and subsequent neoliberalism, the region's militarism/state terror/redemocratization processes), the slighting of which may exaggerate the differences between national experiences and options. While the question of *how* to integrate transnational and local processes and structures is difficult, it is not answered by privileging a priori the particular and national over the systemic and transnational.

The second caution concerns an excessive focus on the hierarchy and its influence within Latin American Catholicism. Among contributors to this volume, Bruneau and Hewitt are among those according greatest significance to hierarchical actions and changes as factors in the eclipse of radical Catholicism. When the assumption of hierarchical importance and/or effectiveness is relaxed even slightly (as in Klaiber's chapter), the demise of progressive Catholicism becomes only one possible future trajectory rather than a foregone conclusion. The liberationist element in Latin American Catholicism may well be in irreversible decline, but it will take more than arguments based on the Vatican and national episcopal appointments to make the case.

*Auburn University* MICHAEL L. BUDDE

**States or Markets? Neo-Liberalism and the Development Policy Debate.** Edited by Christopher Colclough and James Manor. New York: Oxford University Press, 1991. 359p. $69.00.

**The Market and the State: Studies in Interdependence.** Edited by Michael Moran and Maurice Wright. New York: St. Martin's, 1991. 259p. $65.00.

Anyone trying to portray states as generically efficacious agents for betterment of the human condition was an easy target for aspiring "neoliberal" theorists in the 1970s and 1980s. Neoliberalism reunited neoclassical economic thinking and political analysis with devastating effect. Elegant theorizing legitimated policy prescriptions aimed at restoring the market's dominant role in the regulation of human interchange. Myriad examples of failed state intervention provided empirical reinforcement for both the theoretical and the political agenda.

The rise of the "new political economy" is well-known history. More surprising is the apparent speed with which the neoliberal project has been intellectually undercut. If the authors of these two collections are to be believed, the neoliberal resurgence is running out of steam, and the new political economy is aging rapidly.

Two general propositions emerge from these collections: (1) the empirical record of the past decade has failed to confirm neoliberal claims that increasing the sway of the market is unambiguously superior to state intervention as a means of achieving both growth and equity; (2) even if expansion of competitive markets is the aim, the state must be institutionally reinforced, not shrunk or dismantled. These are substantial propositions and both books are well worth reading.

The Colclough–Manor collection is the stronger of the two. Both the individual articles and the collection as a whole are more focused and therefore more convincing. The collection's focus on Third World countries and the international regimes that affect their development defines a clear arena of debate with neoliberalism.

Even independently of its debate with neoliberalism, this collection is a substantial contribution to scholarship on development. Theorists of development tend to gravitate toward analysis of successes with a further preference toward topics like trade, finance, and industrial production where "real" data are readily accessible. This collection has excellent pieces on trade (David Evans) and finance (Stephany Griffith-Jones), but the weighting is toward neglected issues. Agricultural failure gets more attention than industrial success; sub-Saharan Africa, than East Asia; and the provision of social services, than production of steel or autos.

The paper by Mick Moore on irrigation is a model of what each essay tries to accomplish. First, Moore presents a sophisticated review of the new political economy or rational-choice approach to irrigation, an approach that predictably stresses the importance of linking the provision of water to specific fees. Moore then goes on to use the specific case of Taiwan to show that a complex combination of social networks and centralized bureaucracy, not fees, is what makes Taiwan's irrigation system "perhaps the most efficient in the world" (p. 292).

Other papers have a similar flavor. None of them advocate a return to what Robert Chambers calls "neo-Fabianism," a mode of analysis that ignores the likelihood of government failure. Each takes the neoliberal case seriously but shows that neoliberal prescriptions do not live up to the claims of their advocates, while state involvement is often crucial to achieving even the ends sought by neoliberals. Lipton's conclusion with regard to agriculture is typical. It is not just that evidence on the impact of combining "state compression" and "market expansion" in agriculture is "to date mostly neutral or negative" but that at least in agriculture, "risk and information are so structured that growing state involvement is a prerequisite" to the creation of competitive markets (pp. 27, 39).

Colclough and Manor have succeeded admirably in demonstrating that it is time to move beyond neoliberalism. What they have not done is to provide a more powerful successor paradigm. Indeed, the authors' shared concern with the practical problems of policy implementation tends to lead them away from general paradigms altogether.

Colclough sets the tone in the introduction when he argues that one of the principal problems with the new political economy is its failure to acknowledge that "each country circumstance is specific, requiring particular analysis before detailed prognoses can be offered." Robert Chambers is more aggressive, arguing for a "paradigm of reversals" that starts with the "dispersed, diverse and complicating" perspectives of poor rural people themselves (p. 276). While it is hard to gainsay the virtue of focusing on "field-level realities" (ibid.), an approach that hopes to replace neoliberalism's role as the theoretical undergirding for a full gamut of specific policies must itself present a clearly articulated set of general propositions with some claim to universal applicability.

The absence of a successor to the aging "new political economy" is even more evident in the second of these collections, Moran and Wright's; but it suffers from a prior structural problem, as well. It originates in a lecture series commemorating the fortieth anniversary of the founding of the department of government at the University of Manchester. The scope of interests represented in the series, while a credit to the department, makes it hard for the volume to deliver a coherent argument. Contributions range from philosophical engagements with classical political economy (Geras on Smith's conception of property) to specific case studies of industrial regulation (Humphreys on broadcasting). The individual chapters are too short to allow the development of substantial research contributions. Some are quite successful as essays. (Paul Cammack's chapter on the Latin American state, for example, is a very nice synthetic overview of recent thinking.) But pulling an overall message out of the diversity is still a challenge.

In terms of its contribution to the debate with neoliberalism, the high point of the volume is the editors' heroic synthesis in the concluding chapter. Though not quite convincing as a summary of the chapters that have come before, the effort is fascinating in the degree to which its main points parallel those of the Colclough and Manor volume. According to Moran and Wright, one of the central conclusions of the Manchester seminars was that "market exchange depends on state power" and "the 'freer' the economy, the more extensive and powerful the state has to be" (p. 243). Markets and states cannot be conceived as "opposed forms of social organization" but must be seen as symbiotically linked (p. 239).

Taken as a whole, the Manchester lectures, like the Colclough and Manor articles, are clearly at odds with neoliberal efforts to make the contest between state allocation and market mechanisms the principal axis of analysis in political economy. Again like the Colclough and Manor articles, the Manchester lectures provide only vague clues as to what the theoretical and methodological building blocks of a successor approach should be. Peter Humphreys invokes "the new institutionalism" (pp. 200, 216); but having indicated his allegiance, he does not provide programmatic elaboration. Moran and Wright suggest that the market transactions and behavior of firms should be seen as political processes involving bargaining and struggle. Yet they do not take the next step of opening a debate with the contemporary institutional economists like Oliver Williamson, to say nothing of showing how they would improve on such formulations.

Despite the failure of these collections to provide a plausible successor to neoliberalism, they do provide a few enticing hints as to what might be involved in such a project when it eventually does emerge. First of all, such a project will, in good Hegelian fashion, involve a synthetic incorporation of some of the elements of neoliberalism. The Colclough and Manor volume is clearest on this point because it engages neoliberalism most directly. Even the most critical authors take pains to point out that concepts like "rent seeking" must be permanently incorporated into the analysis of development even if neoliberalism's exaggerated claims as to its effects are rejected. At the same time, any successor will certainly be "institutionalist" in the sense of focusing on the consequences of historically robust patterns of social ties and interactions, crucially reinforced by norms and culture, both in and out of formal organizations. Giving this institutionalist side more elegance and bite will be the real trick.

*University of California, Berkeley* PETER EVANS

**Central Bank Strategy, Credibility, and Independence: Theory and Evidence.** By Alex Cukierman. Cambridge: Massachusetts Institute of Technology Press, 1992. 496p. $50.00.

This is a contribution to the "new political economy," that is, the application of economic methodology to political questions. It is also a contribution to classical political economy in that it examines the economics and politics of monetary policy. The unifying theme of the book is, Why do central banks not simply provide enough money so as to make long-term inflation zero? The first three-quarters of the book answer this question through a variety of game-theoretic models, while the last quarter of the book is an empirical analysis of the causes and consequences of central bank independence.

The models presented in the book have been formulated in the technical economics literature over the past decade. Some of the work presented here is new; some is based on Cukierman's professional writings; and some is based on the work of other scholars in the area (Alesina, Rogoff, and Tabellini, among others). These models deal with the problem of time inconsistency, credible commitment, and asymmetric information in the making of monetary policy. All the models analyzed here are squarely in the rational expectations framework, where only surprise monetary policy can have any consequences on the real economy (employment and growth).

The first section of the book presents a number of models that deal with the problem of time inconsistency. At any given moment, the central bank can increase growth and decrease unemployment by a surprise increase in the money supply. This policy is suboptimal in the long run; but in the short run, the bank always has an incentive to engage in such manipulations. The models in the first section are all of this variety, with the central bank having a differing set of motives (increase employment, increase government revenue, deal with balance-of-payments difficulties, and smooth out interest rates).

The larger, second section deals with newer models of information asymmetry. The central bank has a variety of informational advantages, either in knowledge about the economy or, more interestingly, in knowledge of its own objectives. Private economic actors learn about the central bank, but this learning is both imperfect and time-consuming. These models all lead to excess inflation, because the central bank cannot simply announce that it is following an optimal policy and have all agents believe it is truly following that policy.

As noted, while some of these models are newly presented here, the basic modeling approach in the book is well known to readers of the economics literature. Thus, the main gains from this book will come to those unfamiliar with the approach of the new political economy to the study of monetary policy. Such readers will inevitably face a technical barrier in coming to understand the game-theoretic modeling. Cukierman does an excellent job in lowering those barriers, both providing good nontechnical introductions and following a strategy of moving from simpler to more sophisticated modeling. The book is thus very useful to those interested in applications of modern game theory.

Cukierman's choice of models leaves out a number of models of monetary policy that would be of more interest to political scientists. He has only one short chapter on the role of parties and elections, in which he presents the already well-known work of Alesina and Havrilesky. Cukierman does not consider the more overtly political models of Lohmann, Rogoff, or Waller (among others). This is not to criticize Cukierman, who is, after all, writing primarily for economists; but it does make this book less interesting for most political scientists.

The final quarter of the book is completely different. It is a largely empirical examination of central bank independence (loosely tied to the theoretic models developed earlier). Unlike most empirical studies of central bank independence, which are limited to the developed nations, Cukierman studies 70 nations over the last 40 years. Unlike previous studies, he combines the assessments of experts (gathered via questionnaire) with legal and behavioral assessments of independence. All the data collected is presented in the book, which gives the reader far and away the richest source of information available on central bank independence in an impressive variety of countries.

Cukierman conducts a wide range of econometric analyses. The major question in this literature is whether independence leads to lower inflation. The finding here is mixed, with better results for the developed nations. The range of countries analyzed allows Cukierman to study some political determinants of bank independence. There is some indication that political stability leads to increased independence, although (as Cukierman notes) the results explain only a small portion of the variance in independence.

In summary, political scientists most interested in the material of this book will probably be familiar with it through the professional literature. The book provides an excellent introduction to a variety of modern game-theoretic models and can thus serve as an entry into this difficult field. Political scientists will find the section on bank independence interesting but will find that their appetites have only been whetted by the analyses presented.

*University of California, San Diego* NATHANIEL BECK

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**The Making of Social Movements in Latin America: Identity, Strategy, and Democracy.** Edited by Arturo Escobar and Sonia E. Alvarez. Boulder: Westview, 1992. 383p. $59.95 cloth, $18.95 paper.

Study of the recent global wave of transitions to elected civilian rule stressed the role of political parties. Now that the agenda is shifting to the prospects for democratic consolidation, research questions about the process of representation broaden, and the relationships between political institutions and the rest of society become both more important and more problematic. Along with the weakening of political parties in long-standing democracies, the challenge of consolidating fragile democracies has drawn researchers back to Tocqueville's proposition that democratic governance depends on the quality of associational life in civil society. This major collection deepens our understanding of the process through which diverse societal groups in Latin America develop "collective identities" and engage in politics. The 18 essays reflect the editors' interdisciplinary bent, combining Escobar's postmodern anthropological emphasis on the submerged social and cultural networks from which movements emerge with Alvarez' creative political process approach to the interaction among social movements, parties, and governing bureaucrats (see Alvarez' *Engendering Democracy in Brazil* [1990]). The contributors range from senior scholars to graduate students fresh from cutting-edge field research, balanced between U.S. and Latin American research traditions.

The editors challenge explanations of collective action that rely on either structural conditions or assumed interests, arguing, instead, that "culture mediates the movement from structural conditions to social and political action" (p. 319). They offer a remarkably nuanced portrait of the complex processes through which "collective identities are constructed, contested and continually negotiated" (ibid.). The collection tries to bridge the gap that has divided strategy-oriented and identity-oriented approaches. In the U.S. sociological tradition, the resource mobilization and political process frameworks stress how existing groups deal with political elites and opportunities, while European-influenced post-Marxist approaches stress the process through which social groups become actors and try to define themselves autonomously. While most of the contributors fall clearly into one camp or the other, the editors argue convincingly that the two approaches are, indeed, complementary.

This volume represents an important rethinking of Latin American social movement research, which, in the 1980s, often told us more about the analysts' normative hopes than about the actual politics of the movements themselves. Most of the studies are well grounded empirically and offer fresh new case material that will further future theory building. Most focus on the *why* or the *how* of social movements. Contributors show how Peruvian peasants build their own local systems for the administration of justice (Starn) and how Colombian indigenous peoples come to define and redefine their rights (Findji). Others challenge conventional assumptions about social movement autonomy from political parties in Mexico, Chile, and Uruguay (Hellman, Bennett, Schneider, and Canel). Cultural innovations enable Venezuelan ecologists to project themselves politically far out of proportion to their numbers (García). The study of low-income women's movements in Ecuador shows how the boundary drawn in the literature between "practical" and "strategic" gender interests breaks down in practice (Lind). Brazilian homosexual movements redraw the boundaries between public and private but become just as vulnerable to partisan conflict as are less clearly "new" social movements (MacRae). The rich history of Latin America's decade of regionwide feminist conferences ("encounters") offers one of the most evocative analyses of how political identities are socially constructed (Saporta, Sternbach, Navarro, Chuckryk, and Alvarez). Perhaps the most surprising findings show that grass-roots Christian base communities in Brazil, widely seen as the associational web underlying the revitalization of civil society since military rule, reproduce many of the broader societal biases that inhibit participation by the most oppressed, such as illiterates, blacks, and low-income women facing domestic violence (Burdick). As local government is opened up to new forms of participation and accountability in many countries of the region, social movements face the challenge of making the transition from contestation to proposition, as in Brazil (Cardoso). At the same time, local civic movements increasingly attempt to form broader networks to increase their bargaining power, as in Colombia (Fals Borda).

The volume shows that new, more pluralistic forms of interest aggregation are emerging and that while political parties are still able to transform or intervene in social movements, sometimes the parties are transformed by movements, as well. These important trends remained largely unexplored in theory, however. The volume's focus on the making of social movements is both its strength and its weakness. We learn a great deal about what collective action means to participants but much less about participants' impact on political systems.

*Massachusetts Institute of Techology* JONATHAN FOX

**The Brazilian Workers' ABC: Class Conflict and Alliances in Modern São Paulo.** By John D. French. Chapel Hill: University of North Carolina Press, 1992. 378p. $47.50 cloth, $18.95 paper.

**The Workers' Party and Democratization in Brazil.** By Margaret E. Keck. New Haven: Yale University Press, 1992. 315p. $35.00.

**Without Fear of Being Happy: Lula, the Workers' Party and Brazil.** By Emir Sader and Ken Silverstein. New York: Verso, 1991. 177p. $59.95 cloth, $17.95 paper.

At last, we have solid analyses in English of the Workers' party—the definitive study by Margaret Keck plus the overview by Emir Sader and Ken Silverstein. Even before its candidate, Luis Ign'cio Lula da Silva—known simply as Lula—came within 6% of winning the 1989 presidential elections, the emergence of the Workers' party was one of the more intriguing developments of the 1980s. Keck's point of departure is that the Workers' party was one of the most anomalous things to grow out of the transitions to democracy in Brazil and the rest of Latin America in the 1980s. The Workers' party was unabashedly socialist in a world where communist and socialist parties were wilting in postindustrial polities or crumbling along with the Berlin Wall; it was founded by popular-sector groups (and some intellectuals) in a country where elites have always taken the initiative in politics; it maintained party discipline and made programmatic electoral appeals in a political system dominated by parties that were loose, fluid groupings of politicians who depended on personalism and clientelism; and it grew and consolidated itself organizationally in the context of a transition that weakened or destroyed other parties. How was this party possible?

Keck is the only political scientist among the authors considered here; and her book is the only one to evaluate the history of the Workers' party in light of broader concerns, such as Brazil's transition to democracy and theoretical debates on working-class parties. Her explanation of the party's development focuses on two sets of variables: contextual (mostly institutional and political) and organizational (internal struggles and decisions). Rather than assess these factors through a simple chronological account, Keck adroitly organizes the core chapters by substantive themes such as relations between the Workers' party and the unions, state regulation, and elections. The inattentive reader may lose the narrative thread now and again, but the analytic payoff justifies the organization. More than most studies, Keck manages to distinguish the impacts of structure and agency, and the interaction between them. Along the way, she points out the flukes and unintended consequences that generally redounded to the party's benefit. For example, in 1985 it seemed that the "agents" had really misread the "structure" when the Workers' party boycotted the electoral college and denied their support to the popular opposition candidate. At the time, it seemed that the party had sacrificed its future for a principle. A few years later, once the new government had thoroughly discredited itself, the principled boycott started bringing positive electoral returns.

Why was the Workers' party the major political victor of the past decade? Keck offers several answers. Depending on the period, the Workers' party benefited from its strong ties to labor unions, the ability of party leaders to learn from past mistakes, Lula's charisma, the heterogeneity of its supporters, the blunders of the elite establishment, and the indefinition of the party's ideology. At times, these factors strengthened the Workers' party; at other times, some of them weakened it. The paradox of the benefits of ideological indefinition is one of Keck's major insights and one with profound implications for leftists and popular sectors worldwide. Partly by chance, the party's program has always focused more on means than on ends. The goal of the Workers' party is radical democracy, rather than some socialist state or utopia. What radical democracy means varies from situation to situation. But it may be this indefinition and openness that keeps the Workers' party buoyant while leftist movements elsewhere flounder.

One wishes, at times, that Keck had expanded the scope of her penetrating analysis and meticulous research. The bulk of the empirical material and interviews date from 1982–83. Keck was there at the creation and her eye-witness accounts capture the intense excitement, frustration, and confusion of the early years. However (as Keck herself notes in the preface), further research needs to be done on subsequent developments, such as the contribution of the Catholic Church, the growth of the party in rural areas, and the party's appeal to middle sectors. Keck could also have done more to spell out the implications of her study. If her heretofore neglected bottom–up view is necessary for a full understanding of the transition, how must we revise conventional interpretations of democratization? Moreover, Keck categorizes her study as one of historical institutionalism yet does not fully assess its implications for broader debates in this approach.

Overall, Keck's is a wonderful book. The arguments are convincing, the prose, refreshingly clear. (Keck's experience as a journalist apparently limited her ability to write the contorted prose so common in the social sciences.) Keck focuses her analysis on the "foundational moment" of the Workers' party and the "indelible mark" it left. Her study will likely be foundational and indelible for subsequent scholarship on the Workers' party.

*Without Fear of Being Happy* (an awkward translation of the Workers' party slogan in the 1989 presidential campaign) is an openly partisan overview of the Workers' party. For Sader and Silverstein, the Workers' party represents "the most hopeful model for democratic socialism anywhere in South America"; and Lula "is certainly the most important labor leader in Brazil's history" (pp. 4–5). The explanation for the rise of the Workers' party lacks the nuance of Keck's analysis. The underlying cause is the rapid increase in both economic growth and social inequality in the 1970s: "Brazil's notorious poverty offers fertile ground for the PT's growth" (p. 4). Beyond the "obvious" appeal of the party's program, Lula and the Catholic Church are key elements of the explanation. However, if the Workers' party is unique in Latin America, where most countries have notorious poverty, then the explanation for the rise of the Workers' party must be due more to the kind of factors peculiar to Brazil that Keck analyzes. Later, the book stresses, (as does Keck) the amorphous ideology and the amalgam of social movements that characterize the Workers' party. Unlike other leftist parties in Latin America that imported ideologies, defined their programs narrowly, and went off to seek a social base, the Workers' party began with a heterogeneous social base and moved fitfully to define a program (pp. 104–5).

The partisan tone and lively prose (Silverstein is a journalist) make for an appetizing read, but the book is light upon digestion. Sader and Silverstein cover only a fraction of the sources Keck uses. Their references are mostly newspapers and magazines with a sprinkling of Workers' party documents and scholarly books. (The five-page chapter on ideology has no citations at all.) Readers who expect more than a brief readable introduction to the Workers' party will be disappointed.

In contrast, French's empirical coverage is vast and deep and ranges across interviews with labor leaders, local newspapers, police records, reports from U.S. consulates, and the extensive secondary literature published in Brazil. *The Brazilian Workers' ABC* is a dense history of labor organization and politics in Brazil's fourth largest industrial region, the suburbs of São Paulo known as the "ABC cities." While the title might lead a political scientist to expect an analysis of the Workers' party that was born in the ABC cities, for French, a historian by training, the first half of the Twentieth Century is modern.

Like other recent studies, French's is concerned with the terms of labor incorporation. Labor incorporation is the period when industrial workers first gain institutionalized rights to bargain collectively in the industrial arena and to vote and form parties in the political realm. During the dictatorship of the Estado Novo, Get'lio Vargas granted workers the right to belong to unions and to bargain collectively (though not to strike). After his ouster in 1945, Vargas worked to incorporate workers politically into a Labor party.

A great deal has been written on labor incorporation in Brazil, and French takes it as his main goal to debunk what he calls the "corporatist consensus" in most scholarship on populism and Brazilian labor. For French, scholars imbued with this consensus argue that the state effectively controlled labor through corporatist legislation, that labor leaders were more attuned to political and administrative superiors than their members, and that the populism of the 1950s "represented no break" with the prior subordination of labor (pp. 4–6).

French offers, instead, a "new approach," in which he demonstrates "the central importance of the clash of class interests between industrial workers and their employers" but at the same time argues that "the outcome of these struggles [is] determined . . . by the central importance of the workers' alliances with other social classes, groups, and individuals" (pp. 15–16). What this conflict and alliance approach means does not become clear until later in the book. First, labor legislation in the 1930s strengthened a weak Labor movement but did not give the state control of the new unions created under its protective wing (pp. 49–52). Later, "the dramatic entry of the urban working class into Brazilian political life" constituted "a radical break"; and popular support for Vargas became a "powerful current of working-class self-assertion" (pp. 132, 139). French's main point is that populism and state incorporation empowered the working class: the glass was at least half-full.

The book leaves some critical questions (some beyond the scope of the book) unanswered. The ABC region is crucial to any understanding of Brazilian labor, but how representative is it of Brazilian labor generally? Similarly, more comparisons to other countries would have helped determine just how full the glass was: Were workers in Brazil more autonomously organized than their counterparts in Chile, Argentina, or Peru? Many of those who seek to explain the weakness of Brazilian labor start with this kind of comparison. It is important to remember that the glass is half-full; but if it is much more than that, it becomes hard to explain subsequent developments. If labor in the 1950s is really so strong, independent, and attuned to the rank-and-file, how do we understand labor's subsequent weakness and the emergence of new "authentic" union leaders in the 1970s who rejected the old organizations, leaders, and parties? Such remaining questions notwithstanding, French's book does a great service in reminding us that labor incorporation is not a simple top-down operation; that workers are a diverse group and their interests, socially constructed; and that Lula was not the first worker in the ABC to try to build strong unions and political practices that would really serve the interests of workers.

Taken together, these three books greatly advance the discussion of working-class politics in Brazil. French offers deep background on the first attempts to represent the new working class. Sader and Silverstein provide a succinct, accessible introduction to the Workers' party. Keck gives us the definitive account of its founding and formative years. As it gains strength in Brazil's latest political crises, these books become ever more essential reading.

*Princeton University* BEN ROSS SCHNEIDER

**Rethinking the Third World: Contributions toward a New Conceptualization.** Edited by Rosemary E. Galli. New York: Crane Russak, 1992. 198p. $49.50 cloth, $21.95 paper.

This multi-authored volume reflects a profound concern regarding the relevance of the social sciences, both liberal and Marxist, to the explanation of contemporary social change in the Third World. This book was begun with a highly ambitious goal: to create a new "intellectual agenda" by inviting authors from a variety of disciplines to "raise the most important questions that they believed needed to be asked or that were still unanswered with regard to social change and development." (xi) The ultimate product of this effort to rethink development is admittedly more modest, consisting of a collection of separate essays by each of five authors, with any rigorous attempt at synthesis put off for the future.

The topics selected appear as diverse as the disciplines. Rosemary Galli, drawing on her work in international relations, critiques the inability of development theory to see rural producers as anything other than backward obstacles to change. Political scientist Lars Rudebeck presents a detailed analysis of recent experiences of Guinea-Bissau and Mozambique in his argument linking democracy and development. Sociologist K.P. Moseley analyzes the persistence of so-called "primitive" peoples and the dilemmas and threats this poses for current notions of development. Frederick Weaver, an economist and historian, examines the changing dynamics of the process of industrialization in different epochs. And social psychologist Leonard Bloom attempts to reconceptualize and expand developmental psychology in a way that is more appropriate to the Third World.

But although the essays vary widely in form, content, and approach, they do contain some implicit similarities. Perhaps most important, each in its own way critiques the presumed tendency of contemporary development theories of the right and the left to use macro-level, unilinear, ahistorical approaches which encourage stereotyping and oversimplification rather than careful analysis, often with very unfortunate consequences for the poor and the vulnerable. The authors are also sharply critical of the notion of historical necessity, and argue for the need for contemporary development theory to be "rethought," to permit a more empathetic examination and appreciation of the dynamic and unique histories of the peoples of Africa, Asia, and Latin America. Not only will this help to ensure good scholarship, it is argued, but it will also enable social science to play a part in facilitating a more humane development.

Tied in with this argument is the idea that because development is not an inevitable process, there is the possibility of variation and choice. Weaver's historical analysis of industrialization focuses explicitly on this point, showing how societies do not simply retrace the trajectory of others, but in fact forge unique patterns based on the kinds of technology available and the state of social relations. But while choices exist regarding development paths, this does not mean everyone has an equal say. Unfortunately, argues Galli, current development theory tends to be coopted by the political process, serving the interests of dominant political institutions rather than careful scholarship, with researchers unwittingly carrying out the work of the powerful by providing the justification for strengthening the state and marginalizing the rural producers. But in spite of such efforts to sweep away the so-called "primitives" as obstacles to development, their stubborn persistence against all odds leads Moseley to argue that they not only present a fundamental challenge to contemporary development theory, but may also offer practical solutions for the sustainable development of so-called "modern" societies. This theme that rethinking development theory may benefit the so-called developed nations as well as the Third World poor is also present in Bloom's analysis, who argues that by broadening developmental psychology to embrace the needs and visions of the Third World, it may also become more humanistic and practical in industrialized nations.

The above is only a sample of the variety of topics and themes presented by the various authors. But while the book is rich in ideas, it is still clearly a preliminary attempt at deconstructing development theory. Furthermore, many of the arguments, while controversial, are not necessarily new; there are various bookshelves of literature on peasant resistance, populism, liberation theory, participatory management, and transformational politics that raise many similar issues. Furthermore, some important themes, such as the implications of the end of the Cold War, the overwhelming pressure of environmental concerns, religious fundamentalism, and changes in production due to the new technologies of the communications revolution are either absent or only lightly addressed. Finally, while the book focuses on the Third World, the bulk of the references are to Africa, apparently reflecting the research interests of the contributors. But, as Galli notes, this book is not meant to provide a definitive answer. Its purpose is to focus more on *how* to think, rather than *what* to think about the Third World, and she sees this as a way to stimulate readers to begin to work with others to formulate their own hypotheses. Seen in this light, this book does represent an interesting if uncompleted step towards that goal.

*American University* STEVEN H. ARNOLD

**The Price of Health: Australian Governments and Medical Politics, 1910–1960.** By James A. Gillespie. New York: Cambridge University Press, 1991. 358p. $59.50.
**Paying for Medicare: The Politics of Reform.** By David G. Smith. New York: De Gruyter, 1992. 277p. $39.95 cloth, $19.95 paper.

With health care now planted firmly atop the American domestic policy agenda and reform proposals sprouting like dandelions, we obviously need to think hard about both the substance of innovation and the political context in which it occurs. And as we consider substance and context, we have to understand what has come before, both here and elsewhere.

The two very different and quite good volumes examined here provide helpful background of this sort, while at the same time contributing to ongoing political science discourse. In *Paying for Medicare*, David Smith traces two recent, major bouts of American reform: the prospective payment system for hospitals and the Medicare fee schedule for physicians. In *The Price of Health*, on the other hand, James Gillespie spans several decades of health care debate and reform attempts in Australia, ending his chronicle in the early 1960s. What is most striking about these books taken together is that despite an array of differences, major thematic similarities emerge. Chief among these are (1) the enduring power of, and complexity among, organized providers and (2) the persistently thorny problems associated with both cost containment and access enhancement.

Smith shows how the prospective payment system took root—initially as an idea and then as a program—in political soil that in an important respect might have seemed infertile. It was, after all, an intensively regulatory mechanism proposed during the notably antiregulatory administration of Ronald Reagan. Moreover, it imposed considerable uncertainty on a well-organized and widely distributed constituency accustomed to doing business differently. Yet an array of forces made innovation possible. These included committed agency leadership and technical skill, looming fiscal constraints, and the failure of earlier cost containment initiatives. Reform thus occurred in a context of "awareness in the hospital industry, as well as within the government, that fundamental changes would have to be made" (p. 235). Smith demonstrates how implementation required the government to assuage various hospital interests through a complex and continuing series of detailed policy adjustments. As in most realms of regulation, the generic label "hospital" turns out to apply to a wide variety of facilities (large, small, urban, suburban, rural, religious, secular, teaching) each with material stakes in quite parochial conceptions of fairness on any given question. Similar diversity among physicians likewise challenged the Medicare fee schedule; though it, too, eventually passed, steamrollered via the budget reconciliation process.

Gillespie's chronicle also turns heavily on provider preferences and strategies in ways that will ring familiar to American readers. The British Medical Association (BMA—it became the Australian Medical Association in 1962) strove, over the years, to overwhelm competing health approaches (e.g., homeopathy, osteopathy, midwifery); to manage tensions between the general practitioners and specialists within its ranks; and to secure professional autonomy vis-à-vis a succession of national governments. Most significant is the trend toward government-subsidized fee-for-service medicine and away from alternative financing arrangements (e.g., capitation) and the public health perspective. To anyone even lightly acquainted with, say, Paul Starr's magisterial *Social Transformation of American Medicine* (1982), all of this will sound hauntingly familiar. One crucial element of Gillespie's analysis is sure to generate interest among political scientists, for he aggressively contests Ted Marmor's explanation for the politics of medical remuneration that physician political resources are paramount throughout Western nations. Gillespie argues that the "tenuous unity of the BMA during the conflict over national health insurance," as well as the "complex mixture of fee-for-service and capitation practice in Australia and the marked differences in medical organization and remuneration between states" combine to make "Marmor's notions of 'preference' difficult to apply" (p. 168). At the very least, Gillespie is quite persuasive in proposing a complex and nuanced reading of Australian medical politics.

Indeed, both volumes provide a strong sense of variability in health politics. As Smith tells us, the political paths to the prospective payment system and to the Medicare fee schedule differed considerably. The former was crafted early in the first Reagan term, mainly inside the federal bureaucracy, with the enthusiastic backing of a health and human services secretary (Richard Schweiker). The latter emerged much later, mainly through congressional initiative and in the wake of executive-branch deadlock. And unlike the new prospective payment approach for hospitals, the notion of fee schedules was long familiar to, and accepted by, the physician community. As Smith notes, it was largely in scope, rather than concept, that the fee schedule reform broke new policy ground (p. 127). The idea of diagnosis-related groups on which the prospective payment system hinged proved ultimately unappealing in the physician context on both technical and political grounds. There was no hard data to build on and too much apparent risk for both physicians and patients. In the Gillespie account, on the other hand, an American reader must be struck by a state-level variation in modes of practice and in physician ideology that may not find ready parallels in the United States. Perhaps the most interesting variation of all is Australia's unwillingness to emulate Britain's National Health Service.

A great attraction—and corresponding pitfall—of both books (particularly the Smith volume) lies in their attention to detail. The Smith book is good medicine (if not a final cure) for anyone naive enough to believe that health care reform in the United States would be easy if only we would focus our wandering little minds on it. Smith's account of policymakers grappling with uncertain policy impacts, the need to create desirable provider incentives, and the fear of creating overt losses well conveys the flavor of difficult, high-stakes governance. One may profitably approach *Paying for Medicare* to learn some important nuts-and-bolts of health care policy or, more generally, for two instructive case studies in policy innovation and implementation. But be warned: this is not a book for the distracted or casual reader or for the undergraduate making initial acquaintance with the policy process. The amount of dry technical detail unavoidably served up is substantial and perhaps daunting (though a clear introductory chapter on the policy environment and a glossary are most helpful). Gillespie's account is much less burdened in this way but covers far more ground and displays, of course, a cast of players that will be almost completely unfamiliar to all but unusually well informed American readers.

In the end, the primary lesson of both books undoubtedly lies in the widespread and enduring political difficulty of health care policymaking. In both the United States and Australia, the subject has over time spawned a vast assortment of proposals, some comprehensive in nature and some incremental. Near the end, Smith admits that despite all the technical and political ingenuity underlaying them, the prospective payment system and Medicare fee schedule indicate persistent institutional weakness, rather then strength. Both reforms continue the "national predilection for resolving difficulty with some variety of problem-solving that avoids conflict and makes too little of remote consequences" (p. 233). That predilection shows little sign of abating.

*Brookings Institution* CHRISTOPHER H. FOREMAN, JR.

**Bargaining for Change: Union Politics in North America and Europe.** Edited by Miriam Golden and Jonas Pontusson. Ithaca: Cornell University Press, 1992. 344p. $49.95 cloth, $17.95 paper.

This collection of eight comparative studies dwells largely on union "fragmentation," brought on mainly by economic change, recession, restructuring, and new technologies. The fragmentations cited include the decentralization of union bargaining from national to local levels, a change noted in a growing body of studies, but also a less recognized fragmentation often involving rising conflicts among and within unions as they compete or diverge in their responses to market changes. Several articles also elaborate on the nature and adequacy of union strategies adopted in response to change.

Political scientists, the editors say, tend to treat unions as unitary actors—as did the influential two volumes based on the Harvard Center for European Studies research (Peter Lange et al., *Unions, Change and Crisis* [1982]; and Peter Gourevitch et al., *Unions and Economic Crisis* [1984])—while the present volume shifts to a neglected focus on the fragmenting parts of the whole and to the crisis of national unions and confederal peak organizations such as the AFL-CIO.

While the Harvard studies found a belief among union officials that continued union success required only modification of usual practices, the present volume, Pontusson writes, offers "a different picture, one of unions very much on the defensive, divided among themselves, and lacking a programmatic response to far-reaching changes in their political-economic environment" (p. 2).

The volume divides into three parts: national confederations and wage coordination, industrial unions and sectoral change, and local unions and changes at the firm level. These parts often overlap and, like many edited volumes, do not always pursue wholly coherent and integrated themes. Yet, all the articles and editorial comments offer rich treasures of facts, analysis and provocative ideas. Indeed, some of the ideas and questions they raise provoke even further questions in the reader's mind.

Question one: Is "fragmentation" always the best word to describe the various changes the book discusses? ("Fragment" synonyms: "scraps," "leavings," "remnants.") The whipsawing of local unions that force concessionary bidding against one another to forstall plant closings is an obvious case of damaging fragmentation. Some functions being passed to local levels are clearly divisive and bad for unions and workers, but some are clearly good—training for instance, and labor-controlled worker participation. Indeed, some observers may need to reconcile a militantly rank-and-file position with, for example, an unqualified opposition to union-monitored employee involvement by rank-and-filers.

And where is the evidence that the growth of "good" local powers has inflicted serious injury on national unions or peak confederations? As evidence of "good" results, Kathleen Thelen describes a growing rapport between the powerful national German union, I.G. Metall, and the plant-based Works Councils whose powers and bargaining jurisdictions are expanding—a rapport that is strengthening both national and local groups.

Where is the convincing evidence that conflict within labor is bound to inflict serious damage? The Canadian secession from the UAW, mainly over concessions, may have been damaging, but Charlotte Yates' piece on the subject does not make that claim. Similarly, Peter Swenson's discussion of conflict between private and public sector unions in Sweden and Germany ends with the puzzling claim that "labor turns apparent organizational adversity into political advantage. Though it divides labor, the crisis of the welfare state is not necessarily a crisis for social democracy" (p. 76).

Question two: Do unions indeed lack a "coherent programmatic response to far reaching changes in their political-economic environment," as an editor concludes? If so, where is the evidence, and what would better union responses look like?

Question three: Is there a crisis only of labor or is there a crisis of capitalism as well, on which the labor crisis to some large extent depends? "There are reasons to doubt the long-term viability of the world economic boom of the second half of the 1980s," an editor writes, "but to claim that capitalism is in crisis today seems counterintuitive" (p. 2). Time and change overtake the printed word all too quickly, and today we might find a spreading scepticism about the future intactness of capitalism as we know it now. As for labor's crisis, in the United States we may yet see, as we did in FDR's time, how a new and perhaps labor-leaning administration can, almost by the flick of a wrist, lighten the government/employer imposed burden on union growth and strength.

Question four: How do employers and their surrogates in government figure in the labor crisis this volume discusses? It is "only in the United States," Pontusson writes, that the "employer offensive of the 1980s has involved a direct and sustained attempt to eliminate unions altogether—and, in the United States, this part of the employer offensive actually predates the current period of economic restructuring" (p. 2). As seen most emphatically in the United States, the role of employers is relevant to most questions asked about the labor crisis. That role needs to be addressed and made more explicit.

*New York University* PATRICIA CAYO SEXTON

**Radicalism and Social Change in Jamaica, 1960–1972.** By Obika Gray. Knoxville: University of Tennessee Press, 1991. 289p. $42.50 cloth, $19.95 paper.

As the title implies, this book covers the development of radical social movements and thought in Jamaica during the first decade of independence. In this period, the Jamaica Labour party (JLP) government continued the dependent development policies of the previous decade, which relied on foreign investment to produce growth but failed to produce jobs. Thus, the growing wealth of the upper classes was accompanied by growing unemployment and poverty in the lower classes. As earlier works (above all, Terry Lacey's *Violence and Politics in Jamaica, 1960–1970* [1977]) have shown, this produced discontent and violent protest—the Chinese riots of 1965, the West Kingston war of 1966, and the Rodney riots of 1968—in the Kingston ghetto. It also produced a reaction among the intelligentsia that led to the development of black power ideologies, the promotion of African and Afro-Caribbean culture, the Caribbean school of dependency economics, and Jamaican Marxism. As these radical responses to the contradictions of dependent development gained momentum, the JLP government responded, in turn, with repressive measures: police repression in the ghetto, introduction of flogging and long sentences for lower-class crime, withdrawing passports of domestic radicals, denying visas to foreign radicals, banning songs with critical messages, and censorship of publications. However, since escalation to Latin American–style military authoritarianism was out of the question in Jamaica, the government's actions only served to decrease its legitimacy and further fuel the radical movements.

Gray's book is the definitive analysis of these events, replacing Lacey's work, which focused narrowly on political violence. As such, it is also essential for understanding the social origins of the democratic socialist experiment of Michael Manley's People's National party (PNP) government of 1972–80. Gray begins by tracing the development of movements among the Kingston ghetto-dwellers such as Rastafari and the "rude boys," then moves on to the two most important early 1960s radical political organizations, the Unemployed Workers Council and the Young Socialist League (YSL). He moves on to an analysis of the later movements and organizations: the development of black power thought and Walter Rodney's role in it, the *Abeng* newspaper group, the independent trade unions, and the development of the New World Group, the intellectual forerunners to the Caribbean dependency school. In developing his argument, Gray draws on diverse primary sources: daily newspaper accounts, archives of the YSL, all issues of the *New World Quarterly* and *Abeng* for the life of their publication, and a small number of interviews.

Gray's thesis is that though the radical movements of the 1960s could not capture state power, they nonetheless had great influence on the political development of Jamaica by (1) changing the terms of the political debate and (2) directly influencing PNP policy in the 1970s due to their influence on Manley and their direct participation in the PNP both as a party organization and as a government. One sees the influence of the cultural nationalism and African identity in the political style of the PNP and in the shift in culture promoted not only by the government and governmental media but also by other establishment organizations. The critique of dependent development and the JLP government's authoritarian tendencies advanced by the radical political organizations of the 1960s also influenced the terms of the debate and formed one point of departure for the PNP government's policies of the 1970s. The influence of the activists of the period can be seen, among other things, in the continuity of the personnel of the 1960s activists and the 1970s left inside and outside of the PNP. For instance, Hugh Small, president of the YSL in the 1960s, was a minister in Manley's second term. D. K. Duncan, *Abeng* activist, was general secretary of the PNP during most of the 1970s and briefly a minister during the second term. Norman Girvan, chairman of the New World Group in 1967–68, became, along with George Beckford, the leading theorist of the Caribbean dependency school and was chief technical director of the National Planning Agency during the PNP's second term. As Gray points out, this was a matter of influence on PNP policy, not dominance over it. The Left was always a minority within the party and government.

I consciously chose to label Gray's account of the 1960s radicalism in Jamaica an "analysis," rather than a history because he goes beyond a recounting of events. He adopts a class analytic frame but not one in which racial conflict and racial mobilization is reduced to class. His view of ideology and culture is Gramscian; thus, the struggle between social and political groups is a struggle for terrain in civil society. While asserting the possibility of mass organization's affecting social change, Gray is also acutely aware of the constraints that domestic and international structures place on the process of transformation. Indeed, at the end of the book, he argues that even at the height of its influence and political maturity, the Jamaican Left was too voluntarist, underestimating how far such structures limited what could be achieved.

The book does have its limitations. The failure to trace the evolution of the New World Group forward to the development of Caribbean dependency economics is perplexing, since, arguably, that was the single most sophisticated and influential body of critical thought to emerge from the 1960–72 period. The author also surely missed a mass of relevant material by limiting himself to five interviews, when almost all of the figures that appear in the book were alive when the field research was carried out. Finally, I believe that the book would have benefited from an explicit theoretical discussion of the state and state policy in dependent capitalist democracies, given that the author clearly accepts neither the established Leninist orthodoxy nor its pluralist counterpart. However, these minor criticisms are not meant to detract from Gray's accomplishment: He has produced a work that Caribbean scholars will rely on for some time to come.

*University of North Carolina, Chapel Hill* JOHN D. STEPHENS

**An Institutional Theory of Communist Regimes: Design, Function, and Breakdown.** By Antoni Z. Kaminski. San Francisco: Institute for Contemporary Studies, 1992. 414p. $29.95 cloth, $15.95 paper.

**Reinventing Politics: Eastern Europe from Stalin to Havel.** By Vladimir Tismaneanu. New York: Free Press, 1992. 312p. $22.95.

These two books have rather different concerns and foci. Kaminski sets his sights clearly on the institutional components of the communist social order, examining their points of emphasis, oversights, and consequences. This task inevitably entails a reconsideration of the theoretical underpinnings of communism, starting with the works of Lenin. Indeed, two of the book's 16 chapters are devoted to Lenin.

After elaborating on the role of factions and "interest groups" in the communist system, Kaminski argues that succession crises functioned as error-correcting mechanisms, in which some limited degree of learning from past problems was allowed to impinge on the system. But because this learning was limited to the less certain and most transient periods, it was unable to save the communist system from a disintegration that Kaminski sees as sown into the very fabric of the communist system, quite apart from any effort by a political opposition. Kaminski ends his book with a suggestion that the East European countries may be able to stabilize their new systems and achieve a modicum of prosperity, given credits from the World Bank and the International Monetary Fund.

On the face of it, one could say that Tismaneanu's book also examines the forces and factors that led to the ultimate disintegration of the communist system (though his focus is more narrowly confined to Eastern Europe, above all, to the Polish, Czechoslovak, Hungarian, and Romanian cases); and, like Kaminski, Tismaneanu argues that the roots of the precipitous collapse of the East European communist systems in 1989 lay within these states themselves. But beyond this general comparability, the two volumes diverge considerably, beginning with the fact that Tismaneanu considers the political opposition to have been of primary political importance in the final collapse of the communist system, while Kaminski would see the opposition at most as an inevitable symptom of the inevitable collapse of communism. What Tismaneanu has produced is less an examination of the institutional premises of communism in general (Kaminski's focus) than a narrative account of political dissent in Eastern Europe from 1945 to the present. Well over half of the book is focused on the 1980s; and as a whole, the book is more interested in people and ideas (especially those of the opposition) than in events and policies and the consequences of policies. I find myself tempted to say that this is the kind of political history that a philosopher might write.

That said, it would be a mistake to construe Tismaneanu's book as anything like a history of Eastern Europe since the war. Despite its rather general title, it is more specifically a record of the sundry efforts by the democratic opposition in Eastern Europe "to discover a way out of the stalemate created by the post-totalitarian order"—Tismaneanu's term for post-Stalin communism (p. 130). For Tismaneanu, the contest between the opposition and the communist order was a clash between good and evil, light and darkness. Of communism, Tismaneanu writes, "The whole ideology was nothing but a rhetorical mirage, a semantic usurpation" (p. 196). Yet communism *did* have its appeals, and we would be excessively self-congratulatory not to concede that. Forgotten by Tismaneanu are (1) communism's mixed efforts to advance the social and political position of women (which may be contrasted with the unambiguous and unmixed efforts by conservative forces in postcommunist Eastern Europe (especially the Catholic Church) to restrict women's rights and push them out of politics; (2) communism's commitment to hold ethnic and religious chauvinism in check (a commitment strikingly lacking in most of postcommunist Eastern Europe); (3) the positive role played by communism in breaking up the huge landed estates held by private individuals and churches (estates that are being restored, to some extent, in Eastern Europe); and (4) communism's mixed success in reducing the gap between the wages of the managerial and working classes (a gap now widening with breathtaking speed throughout the region). Communism was a seriously flawed system but is being replaced in Eastern Europe not by emanations of goodness and light but by seriously flawed systems with different flaws. Of these flaws, the most grievous is surely the insensitive refusal to accord women equal dignity and status.

*University of Washington* SABRINA PETRA RAMET

**Marxism and the City.** By Ira Katznelson. New York: Oxford University Press, 1992. 320p. $39.95.

In this contribution to the series of Marxist Introductions published by Oxford University Press, Ira Katznelson analyzes what urbanists can learn from Marxism and also considers how Marxist theory can be enriched by paying greater attention to cities. It is an extraordinarily rich book, which can hardly be summarized in a brief review; all that can be done is to mention some of the directions that Katznelson's analysis takes.

In asking what urbanists can learn from Marxist theory, Katznelson looks at Engels' depiction of Manchester in *The Condition of the Working Class in England* as the chief account of urban phenomena by one of the founders of Marxism. According to Katznelson, Engels' analysis of the spatial patterns and residential life of workers in Manchester, more than any other writing by Marxists ancient or modern, illuminates the process of forming a distinctive and politically self-conscious working class, a class "in itself" and "for itself." But though Katznelson finds much of value in Engels' study, he asserts that like the work of later Marxists it is too quick to regard material deprivation and residential concentration as sufficient conditions for the development of revolutionary consciousness among workers. He observes that the poverty and spatial distribution of the working classes in American cities were similar to those of their cousins in the cities of Great Britain and continental Europe, yet with very different political consequences. Class-based organizations were considerably more likely to win the support of workers in residential neighborhoods and factories in European cities than in American ones, a difference that can largely be explained by the differing histories of suffrage extension in Europe and the United States.

Among the contemporary Marxists whose work Katznelson reviews, he identifies Henri Lefebvre, Manuel Castells, and David Harvey as those who have made the most significant contributions to urban studies and he discusses their writings in detail. He criticizes Harvey's work in particular for its "closed" Marxism—its failure to identify the mechanisms connecting structure and agency—and undertakes to fill this gap by examining the role that cities played in the formation of the working class.

This leads to Katznelson's point that Marxism can be enriched by "bringing the state back in" to its analysis of urban phenomena and the epochal transition from feudalism to capitalism. Katznelson argues that this transition involved the decoupling of property and sovereignty, and that cities played a central role in this process by concentrating the resources upon which the construction of absolutist states depended. At the same time, the development of the European urban system in the early modern era was a consequence of the construction of absolutist states. Thus major cities in the peripheral areas of Europe (e.g. Madrid and Vienna) were the "coercion-intensive" capitals of enormous empires, as Charles Tilly has characterized them; major cities in the European core (e.g. Milan and Amsterdam) were "capital-intensive" trading centers; and the largest cities of all (London and Paris) were centers for the deployment of both coercion and capital.

Ira Katznelson's *Marxism and the City* not only is a major work of urban politics and urban history but also makes significant contributions to comparative politics.

The book argues that the separation of workplaces and residential neighborhoods in nineteenth century cities had a greater impact upon politics in the United States than in Great Britain or continental Europe because the early extension of the suffrage in the U.S. mobilized citizens in their places of residence and thus organized into politics the worlds workers built in their urban neighborhoods. The significant differentiation of urban neighborhoods thereby became especially relevant to politics in the United States, thus helping to explain why the class struggle Marxists advocated was even less common in America than in Europe.

*Cornell University* MARTIN SHEFTER

**The Charismatic Bond: Political Behavior in Time of Crisis.** By Douglas Madsen and Peter G. Snow. Cambridge: Harvard University Press, 1991. 187p. $29.95.

This book constitutes an important effort to develop a framework for studying charisma that has cross-cultural applicability. In it, Madsen and Snow have charted a new course for the study of political leadership. It is a brave book in that the authors not only adopt and extend a major concept in historical political sociology, but deign to establish rules of evidence to examine it in a discrete and important case—Peronist Argentina. The authors are studious of brevity—the distillation of the relevant literature is apt and lean; the relationships between proposition and evidence are tightly reasoned; the analysis evinces ingenuity and originality.

The authors submit that charismatic leadership occurs in the context of a collective psychological crisis induced by a rather immediate and abrupt change in personal well-being and pride in self, where there is a strong and distinct perception of the prospect of loss, and where there exists a collective sense of uncertainty about the future. Such leadership, in short, tends to arise where there is a simultaneity of helplessness, of loss of control, of anger and despair, of loss of self-efficacy. Using the formulation of Albert Bandura, such conditions are conducive to the search for "proxy control" that is imputed to the would-be charismatic leader by the affected public.

Charisma is, in their view, "an influence relationship marked by asymmetry, directness, and, for the followers, great passion" (p. 5). The putative leader has proffered influence, but this is not reciprocal; there is no significant mediation between leader and followers either through formal structures or informal networks. By great passion the authors mean "intense devotion to and extraordinary reverence for the leader," (p. 5) the most distinctive defining element of charisma and an element that is found in a wide range of contemporary political cultures and begs for deeper and more systematic comparative work. Proxy control is accompanied by deep affective attachment—by a "bonding with the leader, and also with the group" which "restores a sense of security and of competency, which ultimately may provide the foundation for renewed autonomy" (p. 15). The authors find two stages in the routinization of charisma—the development of structure where key political disciples become interpreters and intermediaries and in the process erode control of the center of the charismatic movements, a not so new idea; and the "dispersion of the charismatic response" where followers invest intermediaries with the affection and hopes initially concentrated and showered upon the charismatic leader, a less well examined idea.

The analysis provides extremely valuable information, insight, and inferences for those interested in the social psychology of Peronism and in refined testing of formal propositions and nuanced statistical applications alike. It is a welcome effort that addresses in a systematic way an important problem that has been poorly attended in recent generations of political inquiry. The authors remind us that Argentinean immigration differed significantly from that to North America in that immigrants, who constituted the urban working class as well as a nascent middle class, tended not to become citizens. By World War II, however, the urban working class was composed primarily of recent agricultural migrants and, to a lesser extent, the children of immigrants. These internal immigrants differed from their earlier counterparts in that they were potential voters whose institutional protection, however dependent, in the form of rural patron-client relationships was not replicated in the urban environment where, during the depression, there was no change in real wages of a burgeoning work force, but where housing shortages became acute and public services and facilities in the new *villas miserias* were practically nonexistent. It was in this context that Peron—first as Labor Minister and subsequently as President—devoted word, imagination, and ultimately resources to the urban poor.

The authors know how to make the most out of available electoral and attitudinal data; they also condense the historical and sociological context in a compelling way. In their ecological analysis of the 1946 elections the authors develop a vivid portrait of the class and locational bases of political support and establish persuasively, among other things, that rather than an industrial labor class, the recent migrant population constituted the most powerful source of Peronist political support. The authors further find differentiation in the electorate with respect to the dispersion of the charismatic response. Employing retrospective interview data from a 1965 survey, the authors find a differentiation between those with high affect for Peron and lukewarm sentiment for the movement (a "Personalist" response) and those with high affect for the movement and negative feelings for the man (an "Organization" response). Personalists are distinguished by their small town origins while Organization types tended to be from metropolitan Buenos Aires; the latter tend to come from Union households, the former not; Personalists tend to score lower on standard class measures than the others; they also include a disproportionate share of women; the Personalists also are less inclined to believe they are efficacious in political life and tend to endorse the proposition that "strong leaders" are needed while Organizationalists are more inclined to support institutional activity, worker-backed candidates, and the collaboration of unions of different ideological orientation. It is suggested that the passion of charismatic identification tends to become transformed into something akin to partisan identification as a way of portraying the public self.

While there are quibbles to be raised about the book—one would have hoped for interview data developed explicitly for the task at hand and to have it as one point in time for longitudinal comparison—attractive qualities far outweigh the few quibbles. The study is an exemplar of how to employ ecological and attitudinal data to test major propositions about political action and relationships. It will be of interest to anyone interested in comparative political behavior and the culture of charisma.

*University of California, Los Angeles* RICHARD SISSON

**Policy Networks: Empirical Evidence and Theoretical Considerations.** Edited by Bernd Marin and Renate Mayntz. Boulder: Westview, 1992. 330p. $42.50.

**Policy Networks in British Government.** Edited by David Marsh and R.A.W. Rhodes. New York: Oxford University Press, 1992. 295p. $65.00.

*Policy Networks in British Government* can be read as a statement of the current state of policy analysis practised by British political scientists, providing a clear statement of their theoretical concerns, and the concepts used to construct typologies of network characteristics. It summarises the results of recent research, and in ten sectoral case-studies tests the utility of two network models. The comparative perspective adopted by all the European and US political scientists and sociologists who contribute to *Policy Networks* allows them to range more widely across sectors and countries. But it is less a statement of the state-of-the-art, than a report on diverse works in progress. Whereas the British contributors present their findings within strict terms of reference, those brought together by Marin and Mayntz disdain a common language, or an agreed agenda of questions, the editors denying that the analysis of public policymaking using the concept of policy network has yet matured into the coherent middle-range (or meso-level) theory convincingly demonstrated by Marsh and Rhodes. They commend the eclecticism which characterises the approaches and methods used by their contributors and reject an imposed framework of analysis. The properties of phenomena named "policy networks," they argue, cannot be derived from the concept; "we must choose what we want them to designate." This is both disingenuous and falsified by the broad agreement among scholars on what policy networks are, and their principal defining characteristics (usefully summarised in Marsh and Rhodes). Indeed several of their own contributors share much common theoretical ground. There is however a disjuncture between their work and that of other contributors representative of a different, sociological, tradition of quantitative network analysis. The editorial problem of achieving or imposing coherence on a collection of disparate seminar papers is never satisfactorily resolved. Laumann and Heinz's mathematical modelling of group-interaction in four US policy domains and Pappi and Knoke on political exchange in labour policy domains in Germany and the US have little in common with the other contributors whose approach is mainly (but not entirely) qualitative and who research and write within the mainstream of political science theorising about policy networks.

A key conclusion of the Marsh and Rhodes book is the inadequacy of the concepts of policy network and community to explain policy change. Central to such a task is the articulation and linking of different levels of analysis, above all how the relation of middle-range theory to grand theories of the state might be handled. Neither book suggests how this might be tackled although Marsh and Rhodes are at least aware that the meso-level concept of "policy networks needs to be clearly located in a number of macro-level theories of the state, and the articulation between the levels needs to be specified." The need to do so is thrown into sharper relief when the concepts are used to compare policymaking cross-nationally, as in the Marin and Mayntz book. To explain the presence or absence of policy networks or other "governance structures" in the same policy area—and their characteristics, modes of behaviour, and policy outcomes—leads inescapably to a consideration of national characteristics and grand theories which purport to explain differences. This is the missing dimension in the structure of the Marsh and Rhodes book.

Without such a comparative perspective, Marsh and Rhodes' contributors might still have contributed to a fuller understanding of the macro-level "setting" within which their identified policy networks operate by adopting an explicit historico-analytical perspective. If the time-period is long enough this enables the analyst to observe the impact of the role of the state (or rather some of its differentiated organizational parts), and of changes in that role, on the formation, composition, and evolution of policy networks, agendas and outcomes. Longitudinal sectoral studies of the kind conducted by scholars writing elsewhere such as Hollingsworth on manufacturing in the US, and Hancher on pharmaceuticals in Britain and France, capture just those dynamic elements missing from most of Marsh and Rhodes' case-studies. Döhler in the cross-national study of health policy networks in Marin and Mayntz is aware of the significance. "By pointing out that the current institutional structure of a policy field has to be regarded as the result of historical course setting, it is no longer sufficient to look at policy outcomes simply from the perspective of pressure group activities" (p. 237). Insisting upon the institutional foundation of policy networks, the analyst employing policy network analysis has also to ask how institutions persist over time, even though their environment may change, and how pre-existing structures delimit the range of possible options. This excellent advice derived from the work and insights of neo-institutionalists such as Krasner, Hall, Hollingsworth, Campbell and Lindberg, and March and Olson, focuses attention directly upon the macro-level of analysis. Disappointingly, while conducting an heroic three-country comparison of the implementation of neo-conservative reform strategies in health policy, Döhler's analysis follows the more predictable lines of analysis followed in the case-studies in the British collection. Coleman's study of monetary policy in Canada and the US alone explicitly addresses the issue of "the extent to which macro-political institutions shape both the formal structural relationships and informal networks in a policy area" (p. 208), and seeks to redress the balance between macro-political and sectoral variables in the study of public policy. He points out that broad institutional factors implicit to such concepts as a state tradition, policy styles or national paths affect the formal structure linking state and society and the relative importance of informal networks. His analysis of the organisation of monetary policy networks incorporates three macro-political variables: the independence of the central bank, its internal structure, and patterns of consultation with private sector actors. This leads toward an analysis of the policy and behavioural norms determining those variables, in an historical setting. By focusing on organisational strategies and on the "rules of the game" which determine the interaction between organisations, it is thus possible to identify both policy and behavioural norms which characterise national politics (macro-level) and particular policy sectors (meso-level). The dynamics of interdependence employing these concepts remains unarticulated in all the Marsh and Rhodes case-studies, and unmentioned by the editors in their otherwise admirable concluding chapter.

Both books make a prior assumption about the existence of policy networks at the sectoral level. Marsh and Rhodes proclaim as a virtue that all their contributors "found" evidence of policy networks. It would have been both interesting and illuminating to have included studies of sectors where there were no such networks, or where they had evolved or been transformed into other types of "governance structures," not simply other types of networks as shown in some of the case studies. A similar assumption informs the structure of the Marin and Mayntz volume.

Any framework for sectoral analysis must provide not only the means to identify both the necessary and sufficient conditions for the emergence of policy networks, and the concepts and language to analyse and describe types and characteristics; it must also account for their absence and for the existence of other types of "governance structures." It must also provide the means to analyse and compare the conditions of their operation, and how relationships are maintained and sustained, disrupted, transformed and dissolved by adaptations to their external environment. To do this it is necessary to understand the historical development of the institutions and structures of a sector, and the broader contextual factors which determine the organisation of the sectoral relationships between (parts of) the State and the sector.

*University of Manchester* MAURICE WRIGHT

**Revolution and Genocide: On the Origins of the Armenian Genocide and the Holocaust.** By Robert F. Melson. Chicago: University of Chicago Press, 1992. 363p. $29.95.

The subject matter of genocide and the Holocaust is of such enormous and awesome scope that it can rightfully claim to be a field of investigation unto itself. Certainly, the volume of work done in this area since the mid-1940s attests to the seriousness of the study of arbitrary taking of life by political authorities.

Studies of the Holocaust fall into four stages: (1) definition and identification of the issues and magnitudes involved (e.g., Raphael Lemkin, Israel Drapkin); (2) statistical and eye-witness reconstruction of the full range of the Nazi terror let loose upon the Jews and other "undesirable" ethnic groups like the Gypsies (Lucy Dawidowicz, Raul Hilberg); (3) studies relating the Holocaust to genocide studies as a comparative effort (Vahakan Dadrian, Kurt Jonassohn, Roger W. Smith); and (4) examination of the meaning of genocide as a social, political, and religious phenomenon in the broad context of established political theory (Hannah Arendt, Helen Fein, Yehuda Bauer, and, recently, Robert F. Melson).

These are by no means neat compartments, and I certainly do not wish to imply that the names given have an eminent domain to the newest claimant for a "dismal science." But Robert Melson's work appears after close to a half-century of intensive research efforts and therefore must be assessed in those terms. Melson's theses is simple enough: Armenians in the Ottoman Empire and Jews of Imperial Germany survived as ethnic and religious minorities until they suffered mass destruction when the two old regimes were engulfed by revolution and war. Indeed, the burden of the thesis is to prove that there is a direct relationship between revolution and genocide.

His is a bold claim, to wit, that the revolutionary régimes that embraced Pan-Turkism and National Socialism engaged in genocide as a policy and as an ideology, respectively. Melson argues further that it was the rise of xenophobic nationalism that fueled the anti-Armenian feelings in Turkey and triggered the anti-Semitism of Germany between 1934 and 1945. This is a work of contemporary European history no less than political theory. Indeed, how far the theory can be accepted, or considered validated, must depend on the accuracy of the historical claims.

Melson's review of the histories of Turkey and Germany contains difficulties of a different sort. The most serious weakness is his assumption that the Kemalist Revolution was a break in attitudes and policies toward the Armenians. It most clearly was not. The Ottoman Empire engaged in massive liquidation of the Armenian peoples between 1894 and 1896; and in 1909, long before Ottomanism and Pan-Islam were abandoned, a second round of massacres took place. The continuation of massacres by the Turkish authorities during and after World War 1 were just that—a continuation.

Vahakan Dadrian, in *Genocide as a Problem of National and International Law* (—curiously not cited), states that "progressive escalation of the level of genocidal killing of the Armenians in Ottoman Turkey through episodic and recurrent massacres in the eras of Abdul Hamit and the Young Turk Ittihadist in particular is a paramount fact" (p. 333). This suggests not that the destruction of nearly half the Armenians living under Turkish rule was trivial but that the assumption of the existence of some sort of benign empire in the past contrasted with a present revolutionary republic simply does not hold water. To resort to an old caveat, what we have here may be a necessary, but hardly a sufficient, cause of genocide. It is plainly wrong to suggest that (in the case of Armenians, at least) the old régime failed to develop or implement a policy to solve the questions or problems created by the Armenian people (p. 247).

The problem with the thesis of the revolutionary source of the Nazi Holocaust against the Jewish people is of a different sort but equally serious. While it is correct to note that the Bismarckian system was a "cunning device ensuring the stability and unity of the German state," the seizure of power by the Nazis proceeded through the Weimar Republic. To speak of "the German revolution that commenced with the fall of the *Kaiserreich* in 1918 and ended with the destruction of Nazism in 1945" (p. 173) is to blur the essential distinctions between two political systems. It might be neat to presume a movement from empire to secular dictatorship, but it does not quite do justice to real history. There is a further problem: while it may be true that there were those who "denied legitimacy" to the Weimar Republic, it was not toppled by some "concordat" of teachers and bureaucrats (as Melson claims) but by runaway inflation and an economic system in shambles following the Depression of 1929.

Whatever the actual course of events in Germany and however one decides to assess Weimar Germany (as interlude or interregnum), the Nazi régime indeed behaved in a way that neither the empire nor Weimar period could ever have imagined. Melson's thesis that a revolutionary seizure of power changed the foundations of society, law, economy, and cultural values (p. 173) did have a tremendous and direct bearing on the fate of Jewish people—not only in Germany but throughout all Europe.

Still, one must wonder whether Melson's thesis is not too simple. It might be argued that not the Nazi revolution as such but the pending doom of Nazism created the implementation of the Holocaust. The destruction of European Jewry essentially took place between 1942 and 1945, after the fate of the Hitler régime was sealed, in military terms. It was after the Wannsee Conference that the demoniacal plot to destroy the Jewish people took on huge momentum. Between 1933 and 1941, the Jews suffered badly; but the death camps—the systematic engineering of massive destruction—had not got under way.

One might argue that nationalist passions deeply embedded in the psyche of the Turkish and German "races" were more directly involved in the massacres than were revolutionary forces. Indeed, the forces of political upheaval inspired and made possible massive annihilation, but it was hardly as if the dogs of war were simply a function of voices of revolution. The question remains what kind of revolutions Melson is talking about. Many revolutions have taken place in which anti-Semitism and anti-minority feelings have been abated, rather than heightened. One need not turn further than the United States for verification. It seems more the case that culture looms larger than politics as such. One might say that in revolutions of compassion, racist and anti-Semitic assaults are reduced (or, at least, held in check) by a common adherence to the law. In revolutions of passion, when law and order are themselves under attack, genocide, such as that of the Armenians and Jews, becomes possible. One wishes that Melson had taken the next step and looked beyond revolution and war into types of revolutions and wars that produce tranquil, rather than destructive, outcomes. Still, *Revolution and Genocide* raises important issues in the theory and history of the terrible assaults against the Jewish and Armenian peoples in the twentieth century. For that alone, it deserves to be read and pondered.

*Rutgers University* IRVING LOUIS HOROWITZ

**Confidence Regained: Economics, Mrs. Thatcher, and the British Voter.** By Helmut Norpoth. Ann Arbor: University of Michigan Press, 1992. 229p. $34.50.

*Confidence Regained* is an important book. It is a sophisticated attempt to link the ups and downs of opinion polls and election results with the ups and downs of the economy in 1980s Britain. It explores whether Thatcher's radical approach to the economy was responsible for her unprecedented success with the voters. Helmut Norpoth is a skilled explorer in the statistical jungle. He knows how to devise elegant equations to reconcile conflicting trend lines. He takes a fresh stand on well-established disputes, notably on the rival claims of the Falklands War and the economic recovery as determinants of Thatcher's 1983 triumph; and he gives a clear analysis of some of the economic developments of the early 1980s.

However, some of his arguments read oddly today. Although the book was published in 1992, apart from the author's own works, hardly anything dated later than 1988 gets into the bibliography; and the fall of Margaret Thatcher, though referred to once or twice, is not fully digested in the argument. Nor, indeed, is the recession that developed from 1988 onward. *Confidence Regained,* with its Miltonic overtones, seems a strange title in a time of Confidence Lost.

Though the book is impressive, doubts emerge. What is not in the index provides the worry. There is no entry for housing or for privatization; for hiving-off or for the civil service; for Arthur Scargill or for the miners; for Scotland or for the Greater London Council; for Cecil Parkinson or Peter Mandelson. The list could be extended. Obviously, the author was not attempting a comprehensive account or British politics in the 1980s. But a book on economic factors that embraces the Falklands War should surely also discuss the impact of the selling-off of nationalized industries and the epic struggle with the unions. Economic determinism employed as a comprehensive explanation of election outcomes fared ill in 1992, both in Britain and America. But no approach to the mysteries of voting should be ignored. Norpoth offers a significant contribution for those interested in interpreting British politics in the 1980s both in the methodology of analysis and in the treatment of some of the substantive issues.

*Oxford University* DAVID BUTLER

**Localism and Centralism in Europe: The Political and Legal Bases of Local Self-Government.** By Edward C. Page. New York: Oxford University Press, 1992. 186p. $52.00.

Unfortunately, few persons are likely to spend $52 for such a thin book. They will miss one of the more original and coherent efforts to plumb the mysteries of center–local relations in Western Europe. Page's study is a synthesis of the work of the ECPR Committee on Local Government, whose joint product, Goldsmith and Page's *Central and Local Government Relations* (1987) contained the descriptions of seven countries. Four years of reflection have clearly been productive.

Page has courageously struck out on his own to forge a new theory of center–local relations. Rather than exterpolating ideas from the recent controversies, he bases his analysis on an intriguing theoretical scheme put forth by the granddaddy of British political science, W. J. M. Mackenzie, over 40 years ago. Remarkably stripped of jargon and unaffected by the many nefarious efforts to quantify center–local relations, Page suggests that we try to update Mackenzie's notion that local decision making can be compared along two scales. His concern is not interaction or negotiation between levels of government but only how local politics affects local decisions (p. 5). The simplifying constraint leads to certain confusions but also enables Page to make some original and provactive evaluations, which, unlike much center–local literature, lead us back to local democracy. Along with Tocqueville, many of us think that that is why we study local government.

The legal scale is described as the "scope of action" of local governments and the political scale the local representative's interest in the center (p. 6). Thus, Page departs from old controversies about shared functions, optimum size and population of local government areas, and the games bureaucracies play with local politicians. Contrary to R. A. W. Rhodes, another expert on British local government who came up with 37 dimensions of center–local interaction, Page really does keep his analysis lean and workable. With unusual candor for theorists in this area of investigation, he admits that his aim is simply to find what "striking contrasts" there are across countries and which of the two scales most affects local decisions. The legal scale, explored in chapter 2, is hands-on decisions delegated by legal machinery to the local level. Whether this is possible outside Britain, where the ultra vires principle of common (constitutional) law prevails, is, of course, debatable. One glimpses the limits of the scales when the political scale is described as the activities where the local politician excercises "discretion."

Theoretically, it is hard to imagine how a person or government exercises discretion if another person or authority is not in a position to give it. France has dozens of laws and decrees about local discretion (and national prerogatives) that lay dormant. Fiscal and financial powers of localities are a minefield for Page's ideas, because national governments compound the problem by exercising considerable discretion themselves. Peel, for example, decided, much to the benefit of the landed aristocracy, that personalty (personal property) in British tax customs did not include incomes. Had he decided otherwise, we could have avoided nearly 20 years of futile debate in Britain over giving local governments an income tax. Effective intergovernmental negotiation is perhaps the most valued skill of local politicians and local bureaucrats.

All of us will admit that major comparative efforts must be content with collaborators whose interests and funding permit their cooperation. Even so, the test of the indexes would be more persuasive with federal cases. In the final analysis, they do enter the discussion. Page observes, "Some principles of the analysis of unitary states do appear to apply to federal systems" (p. 143). Federalism enters by the back door because Page is sensitive to the tradition and history of localism. He argues that Britain and Sweden have distanced national and local elites so that national supervision of local policymaking is weak. Where the linkages of national and local elites are "intensive," national supervision is strong (p. 140). His dichotomy actually conceals two further possibilities. Denmark and Norway have a weak tradition of localism but, in his words, "developed" strong administrative control. One wonders how this outcome is accounted for by the two scales. The unmentioned situation is a strong tradition of localism and weak administrative control which seems to fit nearly all the federal systems. Swiss cantons can simply refuse to participate in national programs. It appears that Page may have a submerged cultural or structural variable that conditions how legal and political features combine.

Like Mackenzie's initial analysis, this study seems heavily influenced by British experience, where "arm's length" policymaking has a long tradition not only in local government but in the governance of nationalized industries, regions, and the National Health Service. Only in a system of governance where national government has been forcing new responsibilities on local government for two centuries might discretion somehow irreversibly and absolutely transfer functions "without the intervention of central government" (p. 21). French departments and Italian regions suffer from no such inhibitions. Belgium and the Netherlands are truly sticky cases and have been described as nations with no periphery. If the old questions about center–local relations come back to haunt Page, it is not for lack of an ingenious and provoking effort to simplify the problem. Every student of decentralization should read this book carefully.

*University of Pittsburgh* DOUGLAS E. ASHFORD

**Shining Path of Peru.** Edited by David Scott Palmer. New York: St. Martin's, 1992. 261p. $45.00.

This anthology is a timely addition to the English-language literature on what is unquestionably the most enigmatic revolutionary movement in the Western Hemisphere, Peru's Sendero Luminoso (Shining Path). The multidisciplinary list of authors gives this book the capacity not only to inform but to stimulate the reader's own insights into how social conditions in Peru combined with the unique character of Sendero Luminoso's revolutionary doctrine to determine the evolution and future trajectory of the movement. Two especially noteworthy contributions of the volume are (1) its microscopic examination of the origins, leadership, organization, doctrine, and tactics of Sendero Luminoso and (2) its depiction of how nonelite response to Sendero Luminoso tactics and doctrine has varied according to local conditions with respect to land, labor, and markets.

Chapters by Carlos Iván Degregori, Gustavo Gorriti, Tom Marks, and Gabriela Tarazona-Sevillano provide meticulous accounts of Sendero's origins, evolution, organization, and operations. These chapters reveal that for Sendero, doctrine is not an explanation of reality but an ideological blueprint according to which reality must be reshaped and violence is the crucible in which Sendero Luminoso seeks to forge this new order. This theme recurs in several chapters as part of the explanation for why Sendero Luminoso has targeted its violence not only against agencies of the Peruvian state but also against grass-roots organizations that insurgents typically seek to coopt. For Sendero Luminoso, such organizations are deviationist and therefore worthy of destruction. Gorriti's chapter on Sendero Luminoso's founding father, Abimael Guzmán Reynoso, argues persuasively that the ideological rigidity that can justify destruction of peasant associations, as well as police stations, is largely a function of Guzmán's personality and his dominance of the organization.

Chapters by Billie Jean Isbell and Ronald H. Berg furnish fascinating insights into how the widely varied local conditions confronting the Andean communities of Chuschi and Andahuaylas resulted in radically different peasant responses to Sendero Luminoso. Berg reminds us that Sendero Luminoso found its most fertile recruiting ground in those Andean peasant communities that had long struggled in the shadow of dominant haciendas. For them, the land reform program of 1968–78 merely converted the haciendas into cooperatives and replaced the *hacendados* with a new class of state administrators. When Sendero Luminoso attacked these cooperatives, they not only distributed the benefits to peasants in the surrounding communities but, in the process, destroyed the dominant authority structure in the community. As a consequence, Sendero Luminoso could more readily impose their own structures of authority in the institutional void created by the destruction of the cooperatives. In contrast, Isbell's field research in the town of Chuschi, where there were no dominant haciendas, revealed the dominant pattern of conflict to be *between* neighboring peasant communities over land boundaries. When Sendero Luminoso resolved these disputes (forcefully) in favor of Chuschi, they won initial support from the community. But merely resolving boundary disputes did not create the same institutional void into which Sendero Luminoso could assert its own authority. Consequently, when they undertook the radical reshaping of Chuschi according to their own ideological precepts, peasants resisted and eventually appealed to the state to establish a military defense post in their midst.

This theme—that the success of Sendero Luminoso in building local support is a function of local conditions—is further elaborated in the chapters by José E. Gonzales on Sendero Luminoso's success in the coca-growing region of the Upper Huallaga Valley and Michael L. Smith on Sendero Luminoso's urban strategy in Lima. Smith's analysis begins with the recurrent premise that Sendero Luminoso is usually less successful in areas where there already exists a well-developed network of grass-roots organizations. However, Smith demonstrates how Sendero Luminoso carefully adapted its organization and tactics to the special political geography of discontent among Lima's barrio residents and thus was able to challenge both the state and the mainstream leftist parties for support among the urban poor. In a similar fashion, José Gonzales presents a carefully crafted analysis of how the indiscriminate violence of Peruvian military and the corruption of local law enforcement in the Upper Huallaga Valley combined with the sheer lucrativeness of the coca trade to create a situation in which Sendero Luminoso was able to present itself as the protector of the coca growers against repression by the state and exploitation by the coca *trafficantes*.

If there is a disappointment in this book, it is the absence of a major theoretical essay that integrates the other chapters by explaining the emergence and success of Sendero Luminoso in terms of the profound changes in Peruvian society brought on by the land reform of 1968–78. The chapters by Cynthia McClintock and Palmer fulfill this role to some extent, but neither chapter is as rigorous as the previously published body of work by its author. However, since their previous works are certainly among the best theoretical works on the origins and strategy of Sendero Luminoso, these chapters are disappointing only by comparison. They do raise the important questions and provide the reader with compelling insights as to how theory might suggest answers to these questions. As a whole, this volume is one that anyone with an interest the patterns of Peruvian politics, the enigma of Sendero Luminoso, or the dynamics of civil violence in general should add to his or her bookshelf.

*Memphis State University* T. David Mason

**The Red and the Green: The Rise and Fall of Collectivized Agriculture in Marxist Regimes.** By Frederic L. Pryor. Princeton: Princeton University Press, 1992. 550p. $59.50.

**Agriculture and the State: Growth, Employment, and Poverty in Developing Countries.** Edited by C. Peter Timmer. Ithaca: Cornell University Press, 1991. 311p. $45.00 cloth, $16.95 paper.

The two dominant ideologies of the century—capitalism and socialism—spawned different conceptions of how agriculture should be organized and what role the state should play in initiating and sustaining that preferred organization. *The Red and the Green* and *Agriculture and the State* show us where socialism took agriculture and where—now that capitalism stands alone—current thinking is on agricultural policy, respectively.

Frederic Pryor's mammoth comparative study begins with the defensible proposition that reorganizing the agricultural sector into large-scale state and collective farms was the most radical transformation of economic institutions implemented by socialist governments. Thirty-three Marxist régimes are identified and employed to illuminate a series of general propositions about agricultural organization under socialism. Five major topics are explored: the important ideological views of the agricultural sector, the origin and development of the major forms of productive units, the internal organization of the farms and their external links with other sectors, major governmental agricultural policies and the their results, and the changes in the organizational structure in recent years.

Pryor uses a wealth of primary and (especially) secondary sources to present a cogent description of how agricultural sectors throughout the world, from the Soviet Union to the Seychelles, were changed by socialism. The book is a true fountain of well-organized information. Complementing the text are maps, tables, diagrams, and research and statistical appendixes. Pryor is an economist; and he uses the tools of his discipline to address selected topics, such as the terms of trade between agriculture and other sectors. He is both cautious and careful, and few will dare quarrel with what he details.

Questions can be asked, however, about what Pryor did not tackle, namely, larger, more difficult issues of the kind that preoccupy sociologists and political scientists. For example, Why were Marxian agricultural policies so assiduously copied in the poorer countries of the world? What has been the relationship between Marxian agricultural policies and political power? These kinds of questions go unanswered. Unlike most economists, Pryor makes use of the comparative method; but he does so to answer important but small questions, not the broad questions of timeless import asked by the likes of Barrington Moore and Theda Skocpol. Also absent from Pryor's study, except for the first paragraph with its note, is an account of the human cost of "adverse" socialist agricultural policies. There are few people in the book, least of all the many victims of collectivization.

Peter Timmer's edited collection of essays is a synopsis of where thinking on agricultural policy stands now, in the aftermath of both the collapse of socialism and the exhaustion of numerous intellectual fads that held forth the promise of rapid agricultural development in poor capitalist countries. Although the focus is on developing countries, the second chapter by Peter Lindert describes two perplexing but historically robust patterns of state intervention in agricultural pricing: (1) the "development paradox," in which rich countries with few farmers transfer large sums to the agricultural sector through price interventions, whereas poor countries with many farmers tax their agricultural sectors via the same instruments; and (2) the "antitrade bias," in which most countries protect their import-competing agricultural sectors and tax their agricultural exports.

The chapters in the collection are on diverse topics, ranging from price policy and trade strategy to rural development projects and the management of food aid. As Timmer freely admits, the individual contributions do not add up to a systematic treatise on the role of the state in agricultural development. There is unanimous agreement with an unspoken conclusion of Pryor's book, namely, that displacing markets is far less likely to stimulate rising agricultural productivity and rural growth than strategies that seek to make markets more effective and competitive. But there is also a shared suspicion of unrestrained "Dickensian" capitalism. Each chapter in the book argues (for its own set of issues) that the state has a role in the long, slow task of agricultural and rural development.

What Timmer and his colleagues have to say is eminently reasonable; but as a sum of its parts, the book lacks analytical punch. But perhaps Timmer is wise in both introduction and conclusion to chastise against generalizing about such diverse issues for disparate countries. Capitalism has many more faces than socialism.

*Princeton University* Forrest D. Colburn

**Contrasting Styles of Industrial Reform: China and India in the 1980s.** By George Rosen. Chicago: University of Chicago Press, 1992. 168p. $25.95.

This is an ingenious book that uncovers a surprising number of similarities in the baseline conditions in India and China as of the start of their respective reform programs (ca. 1980), in the obstacles these programs met, and in their outcomes, both economic and political. The author, an economist who has worked on India most of his life but who has also worked in China, sets out to trace "a comparison of the . . . political economy of the[ir] economic reforms" in the 1980s. He intends "to examine the relationship of the economic reforms to the policy-making system of each country" (pp. 8–9).

The study begins with an overview (regrettably, too abbreviated) of the political, institutional, and ideological system within which economic policy has been made in the two nations and goes on to show how each industrial system functioned in the prereform period, identifying the pressures that led to the reform programs and their goals. It then summarizes the reforms and their results. The chapter on results displays the author's expertise as an economist and is probably the strongest chapter in the book. It is followed by one discussing the interactions between the broader political economic system and the reforms in each case. The final chapter, again, is very short, drawing out implications for other countries and for the future of reform in China and India.

The baseline similarities Rosen points to are the large size of the countries, their predominantly rural character, their takeoffs in output growth beginning in 1950, low per capita income, and regional variations. Both profess socialist ideologies and emphasize equity; and both are highly nationalistic. For three decades before their reforms, both had a pro-heavy-industry developmental strategy, were inward-oriented, utilized economic planning, and had expanded their public sectors. As socialist countries, both stressed support for weak, debt-ridden firms and have been sensitive to employment considerations. What is most important about their shared socialism, for this story, is the weight of the bureaucracy and its system of controls, the power of its officials, and its linkages to the dominant party in both nations. Important differences were the larger private sector in India, China's stagnation in agriculture, and the presence in China, but not in India, of a Cultural Revolution. Both of these latter factors, Rosen shows, were spurs to change in China. Also, where India's economy had a central banking system and macroeconomic controls, China lacked these.

As of about 1980, the leaderships in each became aware of the technological backwardness, inefficiencies, waste, and low quality of output in their industrial systems and their lag internationally. This recognition was an immediate impetus to reform common to both. A problem with the comparative exercise, however, is that Rosen does not clearly spell out what "reform" itself means or whether the term truly fits the programs attempted in both places. The programs he describes succeeded in expanding foreign trade, enhancing enterprise autonomy to varying degrees, heightening domestic competitiveness, increasing the output of consumer goods, upgrading technology, and loosening some restrictions on the private sector. But because the regulatory system remained largely unchanged in both cases (more so in China than he acknowledges) and because of the corruption and inequities that both programs occasioned, the reforms were partial. Failing to install any kind of political change which would either empower benefactors (and thus provide support) or offer meaningful channels for opponents, the leaders closely identified with the programs in both countries became scapegoats, with Zhao Ziyang in China losing his Party chairmanship (in the wake of the Tiananmen protests) and Rajiv Gandhi and his party losing the 1989 elections in India.

The book's strengths lie in showing the obstacles upon which the reform processes foundered in each case—the inequities, corruption, problems engendered by fiscal and budgetary decentralization (for China), and continuing regulatory restrictions (especially for India). It also covers a good deal of ground in summarizing the achievements of the two programs.

Rosen has identified all of the right variables. But he does not get enough leverage from his comparisons, nor does he adequately develop the political connection with economic reform that he set out to show. For instance, he often alludes to India's democratic system and the potential power of elections to shape outcomes, but he does not discuss turnout data: Who votes, and at what rate? We never see what actually transpires in the Indian legislature, nor are we treated to the views of the opposition parties. There is insufficient attention to the specific sources and modes of resistance and support in each case.

How exactly does the presence of a dominant party play itself out differently in a "dictatorship" (Rosen's label for China) from the way it does in a democracy? We are left stimulated to wonder just why, in systems of such apparently different political types, economic reforms end up with similar outcomes and what, precisely, beyond system type, makes reform so difficult. How much, if at all, does political system really matter in executing reform, in the face of socialist ideology, the expectations and commitments it fosters, the institutions that execute it, and the interests that become attached to it?

There are also a number of errors I identified for the Chinese case: for instance, Rosen seems to be unaware that industrial growth was always high (in the range of 10%), even under Mao, so that increasing it was not a goal of the Chinese program (unlike the Indian one); Hua Guofeng's 1978 plan did not include bolstering consumer goods output (p. 50); Deng did not worry about "unacceptable income differences" as a risk of reform (p. 18); and China's readjustment program of 1979–82 was separate from its reform (p. 55).

*University of California, Irvine* DOROTHY J. SOLINGER

**Labour's Grass Roots: The Politics of Party Membership.** By Patrick Seyd and Paul Whiteley. New York: Oxford University Press, 1992. 275p. $67.00 cloth, $27.00 paper.

**Trade Unions and the British Electorate.** By Paul Webb. Brookfield: Ashgate, 1992. 220p. $58.95.

The internal organization and electoral appeal of the British Labour party have fascinated social scientists for over 30 years. Studies range from Abrams, Rose, and Hinden's *Must Labour Lose?* (1960) and McKenzie's *British Political Parties* (1964) down to the two current well-matched volumes. In the 1960s, Butler and Stokes's *Political Change in Britain* (1969) foresaw Labour becoming the natural governing party in the British two-party system. How different things look from the perspective of 1993!

The election of 1970, which Labour lost, was notable for two things: (1) it marked the highest absolute and percentage vote Labour was to receive for the next seven parliamentary elections, through 1992; and (2) it was the end of the two-party system in the electorate (if not at the parliamentary level because of the effects of the single-member district plurality electoral system). Internal ideological squabbles and reorganizations, policy rifts with the trade unions when Labour was in government, and class–party dealignment of the electorate have all been part of the quarter-century of Labour decline, albeit survival as a major party. Seyd and Whiteley, two veteran academic observers of the Labour party, concentrate their analysis on the first problem, with some reference to the third. Webb, in what appears to be the revised version of a doctoral thesis, focuses on the second and third.

Armed with official approval from the central Labour party organization, Seyd and Whiteley conducted the first large-scale survey of the individual membership of the party in 1989–90 in an attempt to understand who the members were; what led them to join the party; what their political opinions were; how active they were; and how the views of members, activists, and Labour voters (from the 1987 British Election Survey) compared. In short, they assess Labour's grass roots in a period in which the roles and views of Labour party members have been questioned. Several accounts of Labour's internal problems have portrayed the membership, particularly party activists, as left-wing extremists.

But individuals constitute only 10% of the total membership of the party. Ninety percent of its membership is affiliated indirectly through the trade unions who give both financial and organizational support to the party. Thus, Webb could claim that he is examining the real grass roots of the party, especially since, as he demonstrates, the organizational links between the unions and the party have not weakened appreciably in the period of Labour electoral decline. Yet trade union members no longer support the party in elections, as they did in the 1960s. Webb reviews and tests several alternative explanations for this phenomenon, using the British Election Surveys from 1964 through 1987.

Both books competently assess the questions they set for themselves. Although they tend to neglect social (in contrast to socio-economic) questions, Seyd and Whiteley have a rich questionnaire, which yields the best picture to date of Labour party members. Socially, Labour members and voters are far apart, with the former having more middle-class attributes. Attitudinally, Labour activists are more leftist than members, who, in turn, are more leftist than Labour voters in 1987. The differences, however, are not large except in the cases of nationalization and nuclear weapons. Furthermore, they find a surprisingly large amount of attitudinal heterogeneity among Labour members and activists and a general willingness to compromise policies (but not principles) in the interests of Labour's winning office. Thus, the authors argue that Labour members and activists are not extremists.

More controversially, Seyd and Whiteley contend that in 1987, in constituencies where party membership was strongest, it benefited the party by generating greater activism and Labour votes, even controlling for other variables that affect the vote. But in attempting to discount a "national media effects" interpretation of the election, they fail to control for the impact of the central Labour campaign through the media—likely to be greater in areas with a larger party membership receptive to it. Also controversial is their analysis of why people join the party, which challenges Olson's well-known "paradox of participation" thesis on the basis of very limited data.

Webb shows that the attitudinal and electoral ties between trade union members, both blue-collar and white-collar, and the Labour party have weakened in the period 1964–87. More troublesome, however, is how to account for these changes, which have been greater among working-class trade unionists than among their middle-class counterparts. Webb finds that the elections of 1970 and 1979 were the crucial episodes in this weakening link. Thus, he comes to rely on short-term reactions to government–trade union relations as an explanation for a phenomenon that has become long-term. Trade unionists, like other voters in Britain, have become more instrumentalist in orientation; and Labour has been unable to recapture their long-term loyalty. Although Webb hints that this may have something to do with the public sector–private sector distinction, he is unable to demonstrate this connection with data.

Thus, Webb finds himself similarly placed to many election analysts of British politics—with the view that "something happened" in the late 1960s to early 1970s that had long-term repercussions for the electorate but whose identity remains elusive. While Webb does a very thorough job of examining alternative hypotheses and different measures of electoral change, his analysis would have been strengthened by a sharper focus on explaining the crucial change elections of the 1970s, rather than so many demonstrations of the disparity of patterns between the 1964 and 1987 elections.

Seyd and Whiteley's analysis focuses on explaining loyalty and voice, but it has little to say about exit. Labour has lost substantial numbers of members since World War II, a trend that was only arrested, erratically, in the 1980s. Nevertheless former Labour party members are not surveyed; and there are no specific questions about the conditions under which respondents might consider leaving the party.

Seyd and Whiteley could also have made more comparisons across time and levels of party organization. Although limited comparisons are made between Labour voters and all voters in 1987, these could be extended back to the Labour "core vote" in 1983 and, in future work, forward to 1992. On an institutional basis, surprisingly few references are made to the studies of the 1970s and 1980s on the socio-economic characteristics and political attitudes of Labour party conference delegates, parliamentary candidates, members of Parliament, selected constituency parties, local councillors, and selected activist groups even though one of the authors was a prominent contributor to this research.

After making their case that increased membership is important for Labour party electoral survival, Seyd and Whiteley end with the question, "How could the Labour party do without people like that?" (p. 220). By Webb's analysis, the Labour party has been performing poorly electorally largely because it has been doing without a large part of the trade union core that once supported it. How can Labour recover? Should it emphasize a larger and more activist membership or a better issue appeal to people whose socio-economic characteristics should incline them to vote Labour? Are these roads complementary or conflictual? Despite their sophisticated analyses, these books leave the dilemma unanswered.

*Oklahoma State University* DONLEY T. STUDLAR

**Democracy at Work: Changing World Markets and the Future of Labor Unions.** By Lowell Turner. Ithaca: Cornell University Press, 1991. 279p. $24.95.

*Democracy at Work* is a splendid contribution to a burgeoning literature seeking to bring workers and the workplace back into the study of political economy—a refreshing antidote to a dominant trend in social science which bids farewell to labor. Turner emphasizes that, contrary to the predominant tendency in the United States, the micro-electronic revolution and fundamental changes in the organization of work that accompany the shift do not always produce a decline in the position of organized labor and workers. By comparing organized labor's response to the challenges of technological change and work reorganization in varied industrial and national settings during the 1980s, he identifies sharp contrasts in the trajectories of labor movements during this critical decade.

Turner's analysis powerfully illuminates a bifurcation in the fortunes of labor movements within advanced capitalism in the recent period, which might be summarized as a case of the rich getting richer and the poor getting poorer. In confronting management's attempt to introduce new technologies and reorganize work, powerful labor movements have been able to maintain or even improve their position whereas weak labor movements emerge even weaker. He focuses primarily on two outliers: the United States and Germany. The U.S. labor movement is characterized by low density of membership and extensive fragmentation. Unions possess relatively meager legal rights and (largely as a result of these factors) have generally displayed an arms-length, adversarial orientation toward management. The United Auto Workers (UAW), among the most powerful American unions, had little say when automobile production and assembly were thoroughly reorganized during the 1980s. In some cases, plant managers solicited union cooperation in the process of planning work reorganization, but they were not legally obliged to do so or much constrained by union pressure. Indeed, the process was generally planned with a view to marginalizing, not involving, organized labor. In (West) Germany, by contrast, a country with a cohesive and powerful labor movement and generally cooperative labor relations, IG Metall (which organizes auto workers) was closely involved in the process of work reorganization, a result of its bargaining strength and legal position. German auto executives brought unions and works councils into the planning process, both because they calculated that they would derive benefits from doing so and because they were obliged to do so as a result of legal constraints and labor's powerful position.

Although Turner does not develop the argument, he suggests that his approach can help explain the causes of America's declining international economic competitiveness. Especially in an era when new technologies demand workers to exercise increased skill and initiative, genuine labor-management cooperation (a precondition of which is that unions have an autonomous base) produces a positive sum outcome for labor, management, and the entire national economy. Contrary to some critics, the explanation for lagging American competitiveness may thus be, not that unions are too strong, but that they are too weak.

Turner's overall hypothesis is that " . . . two critical variables account for relative union success or decline and the stability of industrial relations systems in the contemporary period: first, the extent to which unions, as a broad national pattern, are integrated into processes of managerial decision making, especially concerning work reorganization; and second, the existence of laws or corporatist bargaining arrangements that regulate firm-level union participation from outside the firm" (p. 12). His research strategy is multi-pronged, ingenious, and demanding. The bulk of evidence derives from case studies of United States and German auto firms, at plant, firm, and industry-wide levels. He also tests his hypothesis by analyzing industrial relations in the telecommunications and apparel industries in Germany and the United States, as well as those prevailing in Britain, Italy, Japan, and Sweden, with a particular focus on the automobile industry. The findings from this model of finely crafted comparative research largely corroborate his hypothesis.

Before considering Turner's argument, it should be noted that, through no fault of his, history—in this case, German unification—has outstripped scholarship. The challenge confronting the German labor movement suggests the need to temper undue optimism about its capacity to shape industrial relations. (Turner has recently analyzed changes in industrial relations in East Germany [Ulrich, Klinzing, and Turner, 1993].)

Turner presents his argument with such skill and moderation that the reader may fail to recognize the contentious character of the position that he develops. It is difficult to challenge the claim that the German labor movement has benefited far more from its participatory stance than the U.S. labor movement has by its arms-length, adversarial orientation. Yet Turner frames the issue too narrowly and does not accord sufficient attention to the fact that the labor and socialist movements have long debated the value and terms of participation. He barely analyzes arguments against union involvement in work reorganization and for alternative union strategies.

Further light might be shed on the costs and benefits of union participation, as well as other strategies, by analyzing rank-and-file opinion. For example, how do German workers evaluate union and works council consultation over the shape of work organization? How much do they influence the position of their representatives? Does consultation drive a wedge between representatives, who participate in the process of consultation, and rank-and-file workers? By focusing primarily on relations at the plant and higher levels and failing to analyze shopfloor relations or rank-and-file worker opinion, Turner leaves these important questions unanswered and, indeed, unasked.

Another consequence of neglecting shopfloor relations is to obscure that the book's title is partially misleading. Even the most extensive forms of worker participation in Turner's survey do not embody democratic relations on the shopfloor. At best, works councils and union representatives exercise a mostly *consultative* role. And if the German labor movement partially influences the shape of work reorganization that *results* from technological change, management continues to exercise a near monopoly over decisions about the design of technology and other key elements of the production process. Why dilute democratic standards? Although work relations in German auto plants are far less rigid and hierarchical than those in U.S. plants, even these settings might well be characterized as *Autocracy at Work.*

*Columbia University* MARK KESSELMAN

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**Learning from Gal Oya: Possibilities for Participatory Development and Post-Newtonian Social Science.** By Norman Uphoff. Ithaca: Cornell University Press, 1992. 448p. $34.50.

It is a particularly interesting experience to read Norm Uphoff's book on Gal Oya while visiting Sri Lanka. It has deeply enriched my first trip to this country and my capacity to discuss various aspects of rural development and irrigation organization with colleagues here. This is three books in one. I find the first and third quite convincing but am less convinced by the second.

The first and most important book contained within the covers is Uphoff's observations in part 1 of the step-by-step process of an action-research program that continued throughout most of the 1980s in an effort to increase the performance of the Gal Oya irrigation project in Sri Lanka. I had earlier read several of his famous field notes, as Uphoff had generously shared these with anyone who was interested in this effort to change what had been a hydrologic nightmare into a more productive system. But here one can read the entire sweep of the history of this imaginative and important social experiment.

Any social scientist interested in development should read part 1 of this book. Anyone who intends to change how farmers and officials relate to one another in a developing context should read this part of the book at least twice. It is both a testimony to the imagination, hard work, and creativity of a large number of people and a warning not to presume that fundamental change ever occurs without substantial cost and difficulty. Viewing a decade of process through the lens of twice-a-year visits helps to speed up the camera while still giving the reader a real sense of the motion. Uphoff's honesty about the difficulties they faced and the mistakes they made provides essential insight to how difficult to accomplish tasks like the Gal Oya project are. There is no better journal of a long-standing effort to achieve change than this book.

The second book contained within the covers is a sweeping condemnation of modern social science. Uphoff calls for a dramatic change in the dominant worldview of social scientists, which he finds overly reductionist and mechanistic. There is much to agree with in his critique; but the effort is less satisfying, since it is so diffuse and general.

The third book inside *Learning from Gal Oya* is an effort to redirect the theory of collective action so as to include the possibility that those involved in a social dilemma would reconstitute the structure of the situation they face. This is a more successful effort than his general theoretical critique because he attempts to rebuild, as well as tear down. The stress placed on the possible, rather than the average responses of individuals to collective action problems is well focused. Uphoff would have benefited from a close reading of Thomas Schelling's work on frequency-dependent behavior, since a great deal of Uphoff's argument depends on the distribution of beliefs and behaviors surrounding an individual. Uphoff's stress on the importance of ideas is also an advance over the work of many who think about collective action problems simply in terms of interests and structure, rather than the conceptions that individuals use in relating to one another.

In short, scholars interested in development and in the theory of collective action will find this book of considerable value.

*Indiana University* ELINOR OSTROM

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**Student Protests in Twentieth-Century China: The View from Shanghai.** By Jeffrey N. Wasserstrom. Stanford: Stanford University Press, 1991. 428p. $45.00.

This study focuses on student protest movements during China's republican period (1912–49). The movements covered range from the pivotal May Fourth movement of 1919, which was sparked by opposition to the Versailles Treaty, to the anti–Civil War movement of 1947, which protested China's descent into all-out fighting between the Chinese communists and nationalists. The book mainly deals with student protests in Shanghai, China's largest and most cosmopolitan city; but Wasserstrom also devotes some attention to protests elsewhere and the involvement of other social groups besides students. The organization of the study is somewhat novel: chapters describing particular movements in each decade alternate with thematic chapters analyzing such topics as student tactics, organization, and mobilization and the changing language of protests across the entire period. Wasserstrom was conducting research in Shanghai when the student protests of 1986–87 broke out in that city—protests that were precursors of the much grander and more tragic demonstrations in Beijing in 1989. This front-row seat and opportunity to observe the contemporary relevance of the history that he was studying enabled the author to include a fascinating epilogue chapter on the continuities and contrasts in student protest activity between the republican and communist eras.

The approach adopted in this study is primarily symbolic and theatrical. Wasserstrom feels that viewing student protest activity as powerful political theater is a particularly useful way to understand the phenomenon. By studying the particular symbols used and the "performances" of students (and their opponents), one can learn much about both the sources of inspiration of protest movements and their chances for success. Wasserstrom also draws heavily on comparative materials on student movements elsewhere and on Western theoretical writings on political theater and related topics to illuminate his Chinese cases. The result is an insightful treatment of Shanghai protests that is not framed in a narrow, sinological perspective. To be sure, Chinese student protesters are revealed as engaging in certain prototypically Chinese forms—as when they march together in groups behind banners indicating their organizational affiliation. Yet the author does not see the forms of student protest as static reflections of Chinese traditions. Instead, Shanghai protesters are learning from the success and failure of earlier movements and strategically adopting both traditional and novel symbols in their effort to achieve their goals.

For many readers, the power of Wasserstrom's approach can be seen in the extra insights it provides to the Tiananmen demonstrations of 1989, as described in the epilogue. Many of the journalistic accounts at the time stressed the foreign influences that shaped Beijing's student protests in that year, with calls for Western-style democracy, Gandhian themes of nonviolent resistance, and the erection of the *Goddess of Democracy* statue all figuring prominently. From Wasserstrom's study, however, it becomes clear that most of the symbolic acts of the student protesters resonated powerfully with those used in well-known movements earlier in the century. Even new forms of protest activity, such as the student hunger strike of May 1989, evoked memories of earlier student pledges to sacrifice their lives "to save the country." The ability of the student "actors" of 1989 to evoke memories of the May Fourth movement and later protests is a major factor explaining the widespread popular support the student protesters received. On June 4, 1989, the movement was crushed by the Chinese army, not because they failed to gain popular support but precisely because they had gained so much support.

I find little to fault in this study beside the author's irritating decision to use the current government's system of spelling Chinese terms for his earlier time period. As a result, Chiang Kai-shek becomes transformed into Jiang Jieshi and Sun Yat-sen into Sun Zhongshan. This minor quibble aside, this is a well researched and clearly written study that at times makes the reader feel that he has been transported back to the scene to observe the political theatrics of Shanghai students at firsthand. There is much here to appeal to a variety of audiences, particularly those interested in China, specialists in social movements, and aficionados of political theater.

*University of Michigan* MARTIN KING WHYTE

**Guerrillas and Revolution in Latin America: A Comparative Study of Insurgents and Regimes Since 1956.** By Timothy P. Wickham-Crowley. Princeton: Princeton University Press, 1992. 424p. $59.50.

The unexpected success of the Sandanista revolution has generated an almost romantic fascination with guerrilla movements throughout Latin America. Yet much of the discussion of such movements has remained at the level of polemical harangues, often motivated by ideological concerns having little or nothing to do with scholarship. Fortunately, Timothy P. Wickham-Crowley's *Guerrillas and Revolution in Latin America* has established a new standard for the serious study of Latin America's numerous and usually failed revolutionary movements; and no comparativist who is interested in the study of revolutionary violence and social movements should fail to read this well-crafted work. Wickham-Crowley provides a rigorous analysis of the causes of guerrilla success and failure by focusing on the major guerrilla movements of contemporary Latin America, particularly those that have developed in opposition to the established régimes of Colombia, Venezuela, Peru, Bolivia, Cuba, and Nicaragua. Although oriented toward a "qualitative" comparison and contrast of specific cases, Wickham-Crowley's work combines the rich description of context with an explicit attention to the causal logic of complex historical processes, something not usually found in the study of revolutionary change. Indeed, those interested in methodological precision will be pleasantly surprised by the author's careful testing of competing theoretical propositions and his use of Boolean truth-table logic as a means of establishing the necessary and sufficient conditions of guerilla success and failure.

However, the most important contribution of this book is its careful demonstration of the tendentious character of all reductionist theories of revolutionary change. The author shows in great detail that no single factor, causal condition, or level of analysis can adequately account for the relative victories of guerrilla insurgencies in cases like Cuba and Nicaragua; nor does any single variable explain the lack of guerrilla success in cases like El Salvador and Guatemala. Monocausal analysis simply cannot predict the historical trajectory of revolutionary movements; and Wickham-Crowley proves, at least to my satisfaction, that a model of revolution must incorporate the complex interaction effects of a series of analytically discrete factors and levels of analysis. Specifically, the author argues that guerrilla movements in Latin America succeed or fail as the result of the contextual interaction of guerrilla military power; the depth and extent of rural support for guerrilla movements; and (most critically) the structural characteristics of incumbent regimes that form the target for, and opposition to, guerrilla experiments (pp. 3–18, 302–26). Clearly, as the author points out, a sufficient threshold of military power must be available to guerrilla groups if they are to have any probability of overthrowing what are often deeply entrenched and openly repressive regimes (pp. 60–91). Yet the author sensibly argues that military power is itself determined by other, more important factors, including the nature and level of popular support for the guerrillas' program and the creation of a situation of "dual power" in which institutionally weak, personalist régimes find themselves unable to exercise real control over their national populations (pp. 263–301). Owing to the relative economic and demographic importance of rural society in Latin America, popular support obviously requires substantial agrarian acceptance of the possibility of revolutionary politics, because only agrarian populations can provide guerrilla movements with the manpower and resources necessary for survival (pp. 92–129). Nevertheless, even larger-scale rural support for the guerrilla's cause cannot constitute a sufficient basis for revolutionary success unless it is coupled with a weak and largely "illegitimate" incumbent regime; and the latter condition means that guerrillas will succeed if and only if they can build populist or democratic cross-class coalitions linking rural and urban discontent to guerrilla leadership (pp. 318–26). This last point is, in fact, crucial to Wickham-Crowley's analysis and is perhaps his most important contribution to the theory of revolutionary change; for if true, it means that successful guerrilla movements are likely to be rare outside of cases characterized by corrupt patrimonial regimes with low organizational cohesion and little internal support (e.g., Batista's Cuba and Somoza's Nicaragua). As Wickham-Crowley argues, regimes with these characteristics are unlikely to survive serious guerrilla challenges, even if they have significant external support from the United States. Conversely, highly brutal, coercive regimes are likely to survive insurrectionary challenges if they have sufficient organizational power and at least a minimum of social support from upper- and middle-class groups. Groups like Peru's Sendero Luminoso face ever diminishing chances of success, particularly at a time when democratic experiments in much of Latin America have begun to replace guerrilla radicalism as a focus of popular loyalty.

In addition to providing a more rigorous and theoretically richer account of revolution in Latin America, Wickham-Crowley's work is also worthy of note because of importance for the comparative study of rural politics and "peasant" rebellion. In attempting to explain the agrarian foundations of guerrilla power, the author systematically tests a series of hypotheses about agrarian radicalism drawn from the major theories of peasant politics, including theories of rural social structure, peasant culture, and the much-debated notions of moral economy (pp. 91–153). Wickham-Crowley argues that each of these theoretical traditions can account for some of the sources of rural support for guerrilla movements but that no single theory or set of theories can offer a unique set of necessary and sufficient conditions capable of explaining the presence or absence of agrarian insurrection throughout Latin America (pp. 129, 231–62). By implication, this means that a global or general theory of rural politics must account for agrarian rebellion in terms of factors that are broadly generalizable but rooted in local and contextual conditions.

In sum, *Guerrillas and Revolution in Latin America* is a lucidly written and theoretically important contribution to comparative politics and historical sociology. At last, here is a study of Latin American revolution that has some of the power and significance of the classic theoretical accounts of revolution in Europe and Asia. Although his work might have been further enriched by cases drawn form the nineteenth and early twentieth centuries, Wickham-Crowley has provided a new standard for the comparative analysis of radical politics and political change in the developing world.

*University of California, San Diego* VICTOR MAGAGNA

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# INTERNATIONAL RELATIONS

**Explorations at the Edge of Time: The Prospects for World Order.** By Richard Falk. Philadelphia: Temple University Press, 1992. 255p. $39.95.

**Peace: An Idea Whose Time Has Come.** By Anatol Rapoport. Ann Arbor: University of Michigan Press, 1992. 207p. $29.95.

These are two important books from major peace theorists. Richard Falk remains very much a moral and intellectual force. His long association with the World Order Models Project informs the current work, whose themes are also reminiscent of William Irwin Thompson's *At the Edge of History.*

Falk begins with the postmodern moment. He rejects escapist "Disneyland postmodernism," which he associates with a detached denial of real-world problems and suffering caused by the "depravity and greed" of "abusive elites" (pp. 9–10). He prefers a postmodernism that engages what he perceives to be the growing crisis of "the shadow side" of Western technology, what Lewis Mumford has called "the first planet-wide crisis that has taken place since the last glacial period" (pp. 8, 19). He calls his approach "rooted utopianism" (p. 1). Rooted utopianism aims at global reform through a "citizenship of pilgrimage" along "Highway D-5" (denuclearization, demilitarization, dealignment–depolarization, democratization, and development)" (p. 73). Political space includes resistances, reversals, and enhancements in processes and institutions.

Political culture is one of the book's most powerful themes, one that Falk circles around and returns to again and again. Some of the insights are familiar but are powerfully rearticulated to remind us of profound truths—for example, "Oppositional tactics that premise resistance upon norms endorsed though not upheld by the state tend to give nonviolent resisters the upper hand in normative debate" (p. 113). Laying heavy emphasis on social movements, Falk reminds us that "gaining the normative advantage is . . . central to the prospects of grassroots initiatives" (p. 115).

Falk enjoins us to envision the "long revolution," to extend "law to foreign policy," and to evade sovereignty (pp. 146, 160, 196). He summarizes the argument of his book as follows: "Realist preoccupations operate as a gigantic distraction from the deeper challenges associated with political, economic, and social restructuring in light of the precepts of this new political entity—global civil society!" (p. 227).

Anatol Rapoport's book is part of his lifelong concern with constructive "evolution . . . of systems" (p. 1). He now expands this interest in the direction of the "no¶sphere," or "sphere of knowledge" (pp. 7, 199), following the path of Kenneth Boulding's *The Image.* Rapoport conceives of "peace as an idea that has been dormant for at least twenty-eight centuries" but that may now find in the "no¶sphere" enough nutrients "to sprout and grow" (p. 50). It will flourish through a Lamarckian—Dare one say Lysenkoist?—dynamic of cultural evolution that is different from the processes of Darwinian biological evolution (p. 107). Cultural evolution and adaptation to the environment do not rely on simple, mindless random selection but on learning and communication: "learning by reasoning, learning by imitation, and transmitting what has been learned to future generations" (p. 58).

Like Falk, Rapoport criticizes realism: "It would be a mistake," he says, "to suppose that because the conception of international relations inherent in the 'realist' school of international relations has been congenial to the perpetuation of the war system, it has always been so" (p. 107). War existed long before the present international system. Yet in spite of the long adaptive history of war, Rapoport believes that "coexistence of the institution of war and the human race seems no longer possible" (pp. 107–8), echoing the dark, later thoughts of Albert Einstein and Hans Morgenthau.

Rapoport envisions peace through such traditional realist means as strength and the balance of power, as well as developing institutional means like collective security (p. 162). Like Falk, he also has a more radical "utopian vision" that includes personal and revolutionary pacifist dissent, as well as "peace through law" (pp. 139, 150).

A chapter on "reciprocity" in the "evolution of cooperation" supports his argument, recalling Rapoport's earlier experimental research with Albert Chammah in this field (p. 67). Freudian eros is also present as Rapoport reminds us of "the primal cooperative act: the sex act" (pp. 81–82). Rapoport concludes:

> Enlightenment is the dissipation of superstitions. The thesis developed in this book is that all rationales supporting the continued existence of war as an institution can now be exposed as superstitions. The discrediting of superstition is an irreversible process. This is what is meant by the statement that peace is an idea whose time has come. (p. 199)

Both of these books suggest that peace is possible. Utopias are not fairytales but rich veins of constructive ideas that can gradually come true. The key to a more positive future for the human race lies in the evolution and strengthening of positive cultural norms.

We should like to believe that this all is true. Yet if we are at the edge of history, we are not yet at its conclusion. History, particularly the history of war and violence, has not ended any more than historians have ceased to write. True, the Cold War has been declared over, the Berlin Wall has fallen, and the Soviet Union has ceased to exist as a legal entity in the society of nations. Yet the Gulf War and its sequels are still very much with us, famine ravages Somalia, and we do not know how to deconstruct the multiple and conflicting historical identities and images of Sarajevo: free-fire zone, Olympic village, site for the assassination of the Austrian Archduke Franz Ferdinand and the beginning of World War II.

*University of Colorado, Boulder* FRANCIS A. BEER

**Debt, Development, and Democracy: Modern Political Economy and Latin America, 1965–1985.** By Jeffry A. Frieden. Princeton: Princeton University Press, 1991. 273p. $29.95.

As the title attests, this book aims to be broader in both its theoretical and empirical aims than many recent studies of Latin American political economy. Frieden endeavors to develop a parsimonious explanation of cross-national variation in Latin American countries' economic and political responses to the debt boom-and-bust of the 1970s and 1980s. The book is framed as a test (or, at least, an application) of "modern political economy" defined as the study of "how rational self-interested actors combine within or outside existing institutional settings to affect social outcomes" (p. 16).

The five countries that Frieden examines (Argentina, Brazil, Chile, Mexico, and Venezuela) responded to changes in the availability and cost of foreign loans with economic policies ranging from market-oriented to state-interventionist. These countries also moved closer to either democracy or authoritarianism. The varied responses of this set of countries are a function of different interest groups' policy preferences and capacities for pressuring government. Interest group preferences are, in turn, related to the "specificity" of their members' assets and to the extent of class conflict (i.e., the nature of capital–labor relations). The greater the specificity (the less the liquidity) of an interest groups' assets and the greater "sectoral," as opposed to class, conflict, the more interventionist the government's policy response to changes in the international financial environment. Frieden's argument about variation in political response to the debt crisis is less novel. He suggests that the relative strength of sectoral and class demands on government and government responsiveness to these demands determines whether regime change occurred.

This book makes an important original contribution to political economy literature (particularly that on the politics of economic policy) by highlighting the value of the concept of asset specificity for political and sociological analysis. The attempt to combine sectoral and class conflict in a single argument is also novel. The book is less successful in making an original contribution to the literature on régime change in developing countries or in extending the debate over interest-based explanations of political behavior. Frieden neither endeavors to differentiate his type of interest-based approach from those pursued by Marxists or neo-Marxists, pluralists, neoclassical economists, or others. Nor, for a variety of methodological reasons he himself delineates, can he make a definitive contribution to debates over the relative merit of individual interest versus collective ideology or values as explanations of social reality.

In the case studies, application of the concepts of sectoral (asset-type-based) social division and class conflict is occasionally problematic. For example, it is difficult to discern whether actual class conflict or the perception of class threat is supposed to impinge on policy preferences and demands on government. This is symptomatic of Frieden's lack of clarity in delineating indicators of sectoral division and class conflict. For example, Brazil is characterized as a case of high sectoral division, low class conflict, and low government response to societal demands and therefore considerable state intervention in the economy and régime change. The case narrative presents considerable evidence of government policy, régime change, and market concentration in key sectors (as proof that sectoral demands were strong) but relatively little evidence with which to evaluate the characterization of sectoral division as high and class conflict as low in comparison to other countries. Interviews with business people reflecting lack of concern about the threat from labor are cited, but comparable interviews are not cited in the other cases described. Similarly, it is difficult to have confidence in Frieden's claim that class conflict was more salient in Chile than Argentina, especially when the associated repression was equally brutal in both cases. Furthermore, according to Frieden's argument, Mexico's turn to market-based policies post-1982 would have to reflect a rise in class conflict; yet it is difficult to judge whether the intensity of class conflict varies sufficiently to explain the cross-temporal change in government policy.

Lack of specificity in identifying indicators also hampers evaluation of Frieden's argument about régime change. Frieden argues that where class conflict and class-based demands on government are high and sectoral demands low, régime change is less likely than if the reverse is true. The strength of sectoral demands relative to class conflict, for example, led to régime change in Argentina and Brazil. Labor's threat to capital in Chile explains the continuation of authoritarian rule there. The problems arise (1) in instances where it might difficult to distinguish between sectoral and class demands and (2) more importantly, in measuring the relative intensity of different types of demands in a cross-nationally comparable way. This makes it difficult to evaluate Frieden's argument about régime change in light of alternative explanations of régime change.

These are minor quibbles, however, compared with the magnitude of Frieden's undertaking. In order to move forward theoretically, the field of comparative political economy needs careful monographic work, as well as comparative social–economic histories in the Gershenkronian tradition. There are woefully few of the latter. One can only hope the debate spurred by this book will help inspire further comparative research into the politics of economic policy.

*Yale University* SYLVIA MAXFIELD

**Globalizing the GATT: The Soviet Union's Successor States, Eastern Europe, and the International Trading System.** By Leah Haus. Washington: Brookings Institution, 1992. 141p. $38.95 cloth, $10.95 paper.

The title of this interesting work is somewhat deceiving, in that the author says little about the Soviet successor states and even less about globalizing the GATT per se. The objective of this work is to show patterns in the way the West has responded to efforts by certain East bloc nations aimed at joining the GATT and thereby securing access to the international trading system. Of special interest in this study—initiated shortly after Gorbachev began his well-known reforms—are the accessions of Poland, Romania, and Hungary to the GATT during the periods 1957–1967, 1957–1971, and 1966–1972 respectively. The remainder of the work offers rather sketchy overviews of Bulgarian and Soviet accessions.

The descriptive accounts are interesting in and of themselves, reminiscent of Asseto's *The Soviet Bloc in the IMF and the IBRO* (1988). But this is primarily a study of diplomatic history which does not go far enough to develop a generalizable theoretical framework. Haus shows interesting patterns in the treatment of East block initiatives, which can be broken down into two categories: initial application and terms of accession. In the first phase, the US and EC negotiators were receptive to East European states that showed a willingness to distance themselves from Moscow's central authority. Thus Romania was warmly welcomed—not because its economic program was capitalistic, but because it had denounced Moscow's intervention in Prague in 1968. Bulgaria, on the other hand, was rebutted primarily because of its rigid pro-Soviet foreign policy. Once the application for full membership was accepted, the US tended to oppose the EC's defensive policy of maintaining quotas on East block products, and the outcome was largely a product of intra-Western negotiation. This clear and simple conclusion is abundantly demonstrated in the case studies.

Where the author fails to satisfy is in the discussion of the theoretical implications of the study. This is exacerbated by the unavailability of materials on the post-USSR era (perhaps it would have been worthwhile to wait a year before publication). For example, the case studies reveal a deterioration of US attachment to multilateralism and nondiscrimination which, although mentioned, is not analyzed by the author. Conversely, the EC attitude shifts dramatically in favor of positions previously championed by Washington. When combined with efforts at creating a pan-European economic sphere and the active recruitment of Hungary and Poland (perhaps also the Czech Republic) into the EC itself, we can conclude that there has been a passing of the baton of European leadership from the US to Germany and the EC Commission. The implications of this trend are considerable for studies of hegemonic leadership, post-hegemonic cooperation, and future world order.

Also missing is a discussion of the evolving role of the GATT itself in the negotiation process. Haus seems to assume the GATT system has deteriorated and makes no hopeful reference to the Uruguay Round. But although bilateralism is certainly more prevalent, this is not to say multilateralism is dead (particularly in light of the earlier comments on EC leadership). The author seems to deplore the use of "specific reciprocity" as embodied in GATT dispute settlement panels, but one could argue that enforcement is usually the final stage of a healthy international legal order, and the GATT is finally coming into its own. At any rate, the role of the secretariat is rarely discussed even though its actions seem to have been quite significant at various junctures. It would be worthwhile to develop this project into either a study of international leadership or institutional growth.

Overall, the study is very interesting, tightly constructed, and direct. The policy implications offered at the end of the book are generally sound, although a bit inconsistent with elements of the case study material (Haus advocates using Poland and Romania's accession terms as a model in spite of frequent criticism of their content elsewhere in the book). It should be included in studies of post-Cold War political economy and historical reviews of US EC-East block relations.

*Bowling Green State University* KENDALL STILES

**Refugees from Revolution: U.S. Policy and Third-World Migration.** By Peter H. Koehn. Boulder: Westview, 1991. 463p. $46.50 paper.

**International Migration Systems: A Global Approach.** Edited by Mary M. Kritz, Lin Lean Lim, and Hania Zlotnik. New York: Oxford University Press, 1992. 354p. $68.00.

Mass population movements are a prominent feature of international relations in the post–Cold War era. The growing internationalization of nation–states' economies, including the creation of regional free-trade zones, the transnational impact of the world's communication and media systems, the ever-increasing gap between poor and rich countries, and the proliferation of internal conflicts and state oppression in many regions of the world have led millions of people from the less-developed countries to seek better opportunities and personal security in the richer countries. The number of refugees and internally displaced people in the Third World has mushroomed, and the industrialized states in the North see an increase in the numbers arriving directly or indirectly at their borders. Although the United States has recently increased its immigration quotas, the numbers of people seeking entry continue to escalate. And in Western Europe, despite the imposition of measures to preempt or contain new immigration flows, the number of illegal aliens and de facto immigrants continues to rise. In both the Middle East and Asia, the issues of economic and labor migration loom large: the mass exodus of migrant workers from the Gulf War and the rapid expansion of labor migration throughout Asia are only two recent manifestations of this growing phenomenon throughout the developing world. The number of sending nations continues to expand, and the formation of social and economic networks between sending and receiving states sustain migration flows once they have been established.

Two new books, global in perspective and multidisciplinary in approach, attempt to identify the contemporary trends and patterns of international migration and point to some of the major conceptual, methodological, and data challenges in the field. The central argument of *International Migration Systems* is that international migration flows are not random but are the result of economic, social, and political connections that have been established, often decades ago, between and among nations. In this study, a systems approach to international migration which looks jointly at sending and receiving countries and comparatively across migration systems is employed to analyze these phenomena. Most of the authors in this volume explain international migration primarily by focusing on global or regional inequalities; the economic or social linkages between sending and receiving states (e.g., the movement of capital, goods, technology, and the role played by transnational institutions); and structural changes in labor markets linked to changes in the international division of labor. In this regard, *International Migration Systems* is similar to much of the contemporary literature on international migration focusing on the interactive frameworks linking migration processes and other global processes and the importance of economic differentials and labor market opportunities in stimulating population movements.

However, state actions regarding migration are shaped not only by economic and social factors but also by political factors and by relations with other states. While this theme is treated by a few of the contributors in *International Migration Systems*, it is a central theme of *Refugees from Revolution*. Migrations, particularly refugee movements, are often a deliberate part of state strategy to achieve a foreign policy or security objective. A large proportion of the world's displacements occur as a direct result of political and social revolutions and the attempts to transform social and economic relations and to construct radically different political systems. The over-throw of ruling elites, violent internal conflicts to achieve dominance, and the intolerance of dissent and drive for total consensus characterize most contemporary revolutions and have been the source of many of the world's major refugee movements. Until recently, foreign policy analysts and international relations specialists frequently ignored the interrelationships between foreign, refugee, and immigration policies. A recurring theme of *Refugees from Revolution* is that the flows of refugee and migrants are shaped and stimulated not only by the social and political transformations and actions of sending states but also by other countries' foreign and economic policies. For example, the bipolarity and superpower rivalry that characterized international politics in the 1980s was a massive stimulus to militarization and conflict in the Third World and generated huge refugee flows.

Both studies give full recognition to the global interconnectedness of refugee and migration problems and the importance of understanding the linkages between sending and receiving states. Most of the case studies in *International Migration Systems* adopt a regional approach (South Pacific, Caribbean, Southern Cone, West Africa, and Western Europe) in defining a migration system. Other contributors in this volume examine both the processes linking various countries together into coherent migration systems (networks, labor recruitment policies, and the role of remittances) and the role of policies and regulations that states adopt not only to control the exit and entry of aliens and citizens but also in shaping international migration systems. *Refugees from Revolution* focuses on four national groups fleeing revolution: Cubans, Indochinese, Iranians, and Ethiopians. Using a wide range of research methods and levels of analysis from the individual, to the state, to the international community, Peter Koehn analyzes both the causes and consequences of South–North movements during the Cold War era when huge numbers of people fled revolutions that had domestic origins but were greatly exacerbated by superpower intervention, especially the United States. *Refugees from Revolution* analyzes each phase of the refugee-migrant cycle—from its genesis, to the search for safe asylum, to resettlement and adaptation in a new society, and finally to repatriation—and defines the considerations necessary for more coherent U.S. and intergovernmental policymaking in the future.

The major lesson of these two valuable studies is that there exists a great need to consider the study of international migration from a broader perspective and knowledge base than has previously been the case. International migration is a structural characteristic of broad social, demographic, technological, economic, and political changes occurring in the international system. Migration and refugee policies involve much more than controlling borders; protecting labor markets; concluding bilateral and multilateral agreements; defining various categories of migrants; adjudicating claims for asylum and refugee status; resettling or repatriating refugees; and determining which intergovernmental, national, or nongovernmental institutions should offer assistance and protection. Migration and refugee policies intersect with important geopolitical and regional interests and are part of a state's broader foreign and security policies. For example, Western economic and humanitarian assistance policies for the former Soviet Union, Western policies toward the recognition of new states and toward the question of intervention in the internal conflicts in Somalia or Bosnia, and U.S. housing loan schemes for Jewish immigrants to Israel from the former USSR are all part of a comprehensive refugee policy and simultaneously affect both refugee flows and regional security. To be effective, therefore, migration and refugee policy must go beyond traditional immigration control and humanitarian measures so that managing migration and refugee pressures becomes a part of the West's central economic, political, and security objectives. Similarly, researchers and scholars need to examine the causes and consequences of population movements within a broad transnational and multidisciplinary context that takes into account political and strategic determinants, as well as economic and social factors.

Any future research agenda will also have to address the linkages between international migration and trade, aid, foreign investment, and human rights policies. Numerous questions about the relationship between migration flows and the policies that receiving states might adopt to stabilize population movements and reduce these flows remain unanswered. For example, what is the impact of trade liberalization on migrant-sending countries, and under what conditions can the expansion of trade reduce the economic incentives for emigration? By providing assistance to postcommunist states to ease the transition from planned economies and state paternalism to market economies, can Western governments alleviate the short-term economic hardships for the unemployed and prevent them from seeking better opportunities abroad? By linking human rights to aid and foreign investment in some countries, especially with respect to the treatment of ethnic minorities, can Western democracies improve domestic political conditions so that people can remain at home? For the most part, there is insufficient understanding about the conditions in sending countries in relation to migration and little attention to what policies might change the conditions contributing to both political and economic emigration pressures. Moreover, every policy option creates political and economic trade-offs for the international community; and in some instances, policies may have unintended or undesirable consequences. For instance, an international policy that lends support for human rights and promotes the rights of ethnic minorities will inevitably involve states in other country's domestic affairs and contribute to internal unrest, which may lead to new refugee crises. A greater understanding of the relationships between migration and trade, development, and human rights policies is key to the success of future policy in this area.

Recent mass population movements in the Third World and in postcommunist states in East Europe have also raised new questions about the sanctity of borders, the nature of sovereignty, and the right of external intervention. Policymakers and scholars need to know what political and economic changes are required in the international system to deal with these issues more effectively in the future. At present, effective strategies for resolving conflicts generated by the breakup of the nation–state are lacking. The international community needs to address how to intervene to prevent refugee flights within or across international borders and how to provide assistance and protection to internally displaced people when their governments object to intervention or in situations, such as Somalia or Liberia, where it is impossible to determine the legitimate government of the country. Failure to address these situations is likely to result in massive displacements, as can be seen not only in the former Yugoslavia but also in the former USSR and parts of the developing world.

During the post–Cold War era, refugee and migration policy, once a low-policy issue in the West, has become a high-policy issue and is now a regular subject for discussion at the level of heads of states and foreign ministers and at G-7 meetings. Clearly, we need more multidisciplinary immigration and refugee research such as *International Migration Systems* and *Refugees from Revolution* to understand how these issues will affect the course of international and regional politics in the years ahead.

*University of Notre Dame* GIL LOESCHER

**Energy and the Soviet Bloc: Alliance Politics After Stalin.** By William M. Reisinger. Ithaca: Cornell University Press, 1992. 184p. $34.50.

When it came to levers of control that the USSR was able to exert over Eastern Europe in the communist period, Moscow brought to bear its military might, its security network, its ideological preeminence, and its *nomenklatura* system, which extended deep into every capital's corridors of power. And when all of this failed, as William Reisinger richly demonstrates in his new book, they also tried to use East European dependence on Soviet energy to maintain their dominance.

Reisinger's book contains a great deal of information about energy flows, capabilities, pipelines, shortfalls, and pricing. It therefore serves as a useful addition to the earlier literature on energy and Soviet–East European trade provided by PlanEcon, Campbell (1968), Marrese and Vanous (1983), and Hewett (1974).

Its primary contribution, however, is not in the discussion of energy but in the analysis of bargaining behaviors and strategies between Moscow and East European states on energy issues. Reisinger assumes that East European states possess independence on substrategic policy issues to pursue their own interests; and he uses energy negotiations to demonstrate the influence East European negotiators were able to bring to bear.

The book takes Keohane's (1984) basic distinction between harmony of interests and cooperation as its starting point in assuming that the USSR did seek cooperation with Eastern Europe by trying to take their interests into account in the energy field. He provides a chapter on the political economy of that relationship broken down into basic historical periods covering the early agreements (1956–63); the era of bilateralism (1963–71); the efforts at multilateralism (1971–74); the response to oil price shocks (1975–81); and the final period of seeking, but ultimately failing, to find new solutions (1981–89).

In chapter 4, he examines energy trade patterns and bargaining behavior to address the fundamental question: Since it can be demonstrated statistically that the Soviet Union did not act in its economic self-interest to maximize economic return for its energy exports to Eastern Europe, what interests did Moscow serve? He makes a highly credible case that energy was sold in exchange for alliance cohesion, foreign policy cooperation, defense spending and coordination, and general East European acquiescence in Moscow's global political strategies.

After providing aggregate analysis of this hypothesis, Reisinger writes two detailed and interesting chapters designed to demonstrate the extent of bargaining which took place: the Polish–Soviet negotiations of 1971 and the Soviet–East European bargaining over energy prices and trade terms in 1975.

Both chapters provide some interesting insights into the mix of political and economic interests that were at stake in these negotiations. Both are limited by the paucity of data (not the author's fault) but also the overreliance on two-by-two game matrices, which simplify the subtle and complex relationship that Reisinger acknowledges existed in both sets of negotiations.

Nevertheless, the book provides an extremely useful and timely contribution not only to the field of Soviet–East European relations but also to the field of political economy and bargaining.

*University of Maryland* KAREN DAWISHA

**Governance without Government: Order and Change in World Politics.** Edited by James N. Rosenau and Ernst-Otto Czempiel. New York: Cambridge University Press, 1992. 311p. $59.95 cloth, $17.95 paper.

There is no doubt that the distinguished editors of this volume are sensitive to the shifting priorities of a post–Cold War world. By focusing on the newly fashionable idea of governance, there is intended a disjunctive look at the evolution of international relations from its antecedent preoccupations with balance and distributions of power, bipolarity, and deterrence. The new conceptual coordinates emphasize interdependence, world order, institutionalization, and international cooperation. At the same time, the 10 contributors to the volume are mainstream political scientists, notable for avoiding both utopian and journalistic approaches to the international political process; that is, the volume neither attempts to prefigure a beneficial sequel to the state system nor is caught up in the immediacies of interpreting the ending of the East–West rivalry and the collapse of the Soviet Union.

In a sense, the importance of the volume lies in its rootedness in the complexities of "the real" as perceived from a variety of perspectives. Amid these complexities, most of the contributors (and certainly the editors) push the thesis that change of systemic significance is probably occurring. James Rosenau in an opening, introductory chapter poses the question in relation to the degree to which observers regard recent developments as undermining the territorial state as the dominant actor in international life. In effect, if changes are merely reconfiguring geopolitics, then the appropriate conclusion is one of continuity; but if these change are fundamentally eroding the capacity and competence of states (as the editors seem to believe), then it is time to bring the curtain down on the Westphalian stage of world order and offer a new conceptualization to students of the subject, a position vigorously advocated in Rosenau's *Turbulence in Global Politics* (1990).

*Governance with Government* is the second of a projected three volumes. The first volume—under the same editorship but more abstractly conceived—was published in 1989 under the title *Global Changes and Theoret-*

*ical Challenges*. We are told that the current volume benefited from no less than five meetings over a period of four years. The word *benefited* must be taken with a grain of salt. Apart from the editors, the contributors seem unevenly concerned with the central motif of "governance": some treat it mechanically as a point of departure, and others seem oblivious to the mission. (For example, Oran Young's competent chapter on the relative effectiveness of international institutions could be included in any book of readings on the world political system published in the last 20 years.)

The editors gather an intelligent group of specialists, parcel out a useful set of assignments, and end up with a set of loosely linked chapters. Each is well worth reading on its own and, I think, only on its own. There is very little, if any, cumulative effect achieved beyond the banal one that changes are happening and that their effects are challenging particular states to adapt their ideas and behavior and, more problematically, that these changes may generate new structures that should alter our basic mindset about the character of world order.

Perhaps the most notable and illuminating aspect of the book is entirely fortuitous, tucked away in a note to Robert Cox's chapter, which strikes me as the most original and enduring contribution in the volume. In the course of critically discussing Waltz's neorealist view that the structure of world order is established independently of the views and behavior of small and middle powers and that only leading states count, Cox disapprovingly quotes Waltz's derisive quip: "Denmark doesn't matter" (p. 114, n. 21). What, of course, makes this somewhat arrogant aside irresistible is that in the meantime, Denmark has forced Waltz to bite his tongue; clearly, no one now would dare suggest that Denmark does not matter in light of the Danish role in derailing the Maastricht treaty process, which only a year earlier had been widely heralded as an assured and dramatic step on the road to Euro-governance. There is more to this point than the coincidence of an embarrassing example. The volume as a whole and the editorial tone in particular suffers from a late modernist pretension that the cognitive map of specialists can somehow capture reality. Instead of pausing to wonder about perceptual failures attendant on the inability of the discipline to anticipate the end of the Cold War and the disintegration of the Soviet Union, the undertaking proceeds merrily to announce that the new circumstances of international life warrant reinterpretation, as if the old circumstances had been previously understood. This disciplinary "oversight" was not a matter of a surprise development but represented the overnight collapse of the intellectual framework that had guided academicians and policymakers for decades and was expected to last indefinitely.

There is no doubt that the contributors to this volume offer intelligent commentary on several critical facets of international life, including strong chapters on statism (Mark Zacher), international political economy (Thomas Biersteker), transnationalism (Janice Thompson), and European regionalism (Linda Cornett and James Caporoso). However, I am somewhat disappointed. There is neither self-criticism of huge recent explanatory failures nor engagement with either the great normative challenges of our time (oppression, environmental decay, poverty, civic disorder, ethnic intolerance) or the great hope, namely, the emergence of transnational social forces dedicated to a sustainable, equitable, and democratic human future. Governance without government—a worthy goal, as well as a partial reality—depends on vision, insight, and compassion at least as much as it depends on evidence and analysis. Let us hope the third volume of the series will fill this void, but I would not count on it.

*Princeton University* RICHARD FALK

**American Intellectuals and African Nationalists, 1955–1970.** By Martin Staniland. New Haven: Yale University Press, 1991. 310p. $30.00.

This is an amusing and disturbing book. It is amusing to read the many simplistic ideas, false impressions, and mistaken hypotheses that American intellectuals (including professional Africanists) have held about Africa during the 15 years covered by this book.

Among Africa watchers of a liberal political persuasion, many at first compared decolonization in Africa to the American Revolution. They saw African leaders as modernizing democrats. Their hopes were disappointed during the middle of the 1960s after the chaos and conflict that followed decolonization in the Congo, as one military coup followed another in Africa and politicians began diverting national resources to promote their political or personal interests. Some liberals rationalized the outbreak of authoritarian rule in Africa in much the same terms as did the new African autocrats: one-party states were necessary for national unity, nation building, and rapid economic development. Other liberal intellectuals, disillusioned that their hopes for the continent had so quickly proven false, turned away in despair and focused their attention on other issues of the day, including the war in Indochina and civil rights at home.

The leftist intellectuals (above all, Marxists) confronted theoretical problems interpreting events and conditions in Africa. What was the role of class in these new societies? How important were ethnic or tribal loyalties? Class analysis was at the heart of their worldviews, while tribalism was often regarded as a camouflage for economic interests. And where was a group of truly revolutionary potential to be found on a continent where there were very small working classes and a limited bourgeoisie?

The description of African American views toward Africa presents a portrait of disinterest and diffidence except in regard to decolonization (which African Americans supported) and racially based politics in the white minority governments of Southern Africa (which they opposed). There is no doubt that African American intellectuals are far more engaged with African affairs today than 20 years ago. Yet the signs of rising interest not just in race politics or decolonization but in the continent as a whole and U.S. engagement there were evident by 1972, with the conflict in the African Studies Association in the United States and the response in Congress, led by Congressman Charles Diggs, to the famine in the Sahel. One is left unsatisfied by this chapter.

Perhaps the least-studied group of American intellectuals, at least in regard to Africa, are conservatives. This is an interesting chapter, pointing out that some conservatives (e.g., William Buckley) regarded Africans as people who "simply do not use their minds," while many other conservatives were doubtful that Africans could overcome tribalism and tradition to create modern, democratic, prosperous societies. But mostly, they spent their time attacking liberal interpretations of African politics. Surprisingly, the author avoids the question of racism, overt or covert, in influencing American conservative views and does not really address conservative attitudes toward South Africa.

This is a useful summary and analysis of thinking on the part of these various groups vis-à-vis Africa. It is striking how many of their own attitudes and biases they brought to their commentaries on Africa; how little effort they made to understand African politics and societies by approaching them with an open mind, acknowledging their own ignorance; and how very wrong they were in their interpretation of the region. The disturbing aspect of this book is the possibility that these problems remain with us today as we try to understand the economic problems and political changes in Africa and advise Africans on how to deal with them.

*Georgetown University* CAROL LANCASTER

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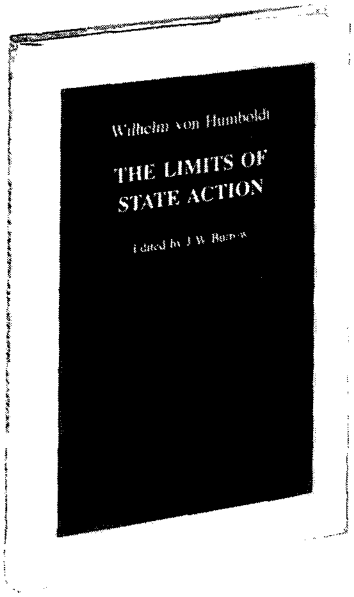

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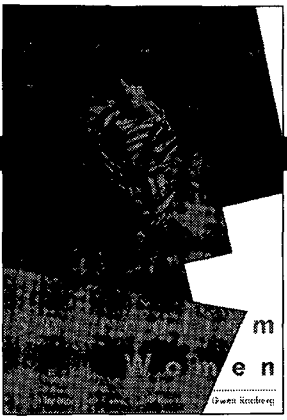

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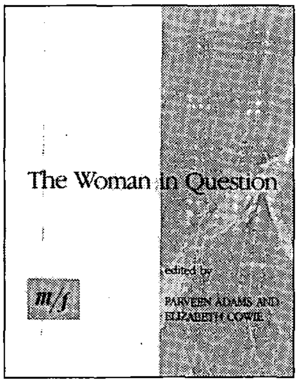

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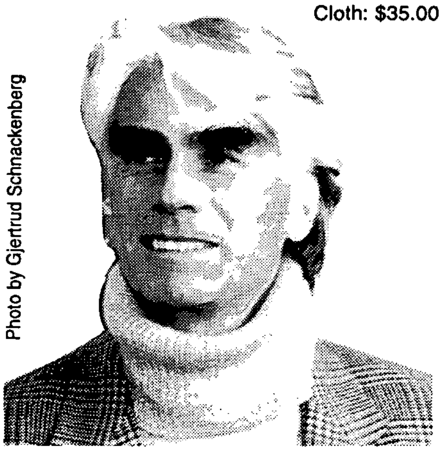

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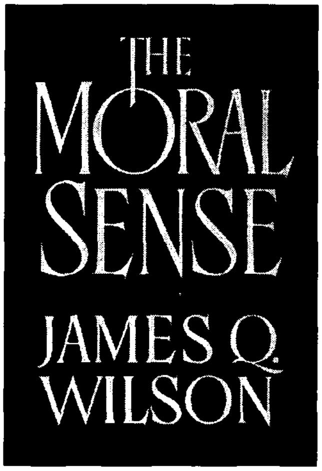

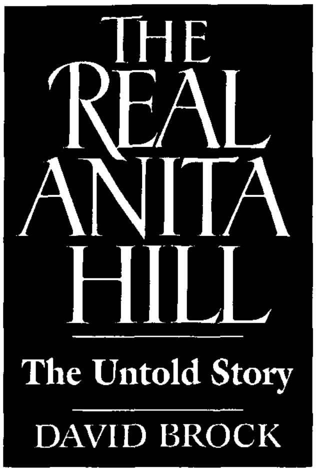

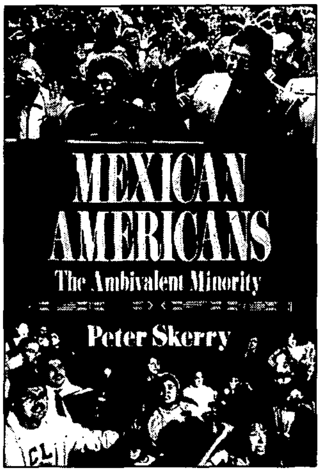

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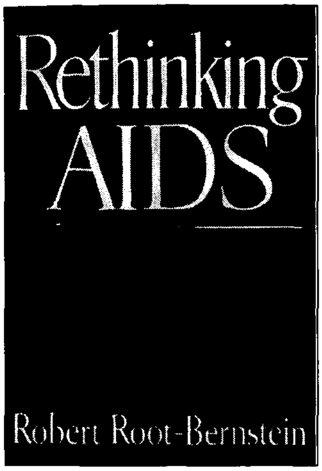

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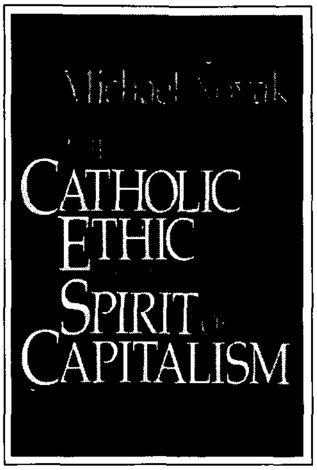

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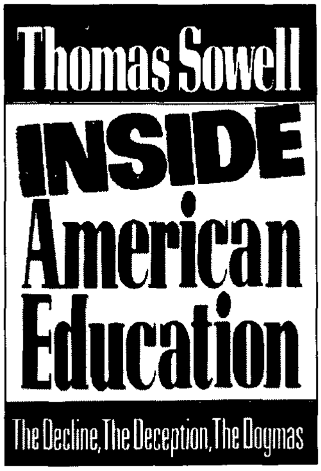

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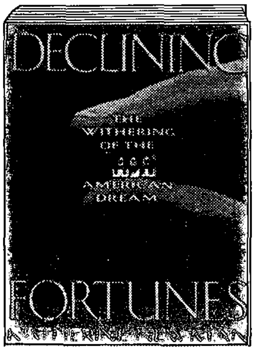

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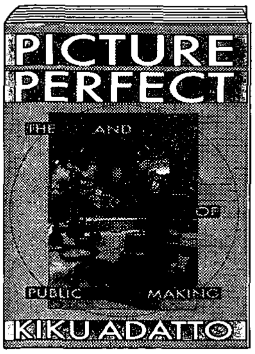

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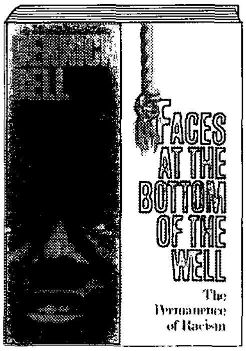

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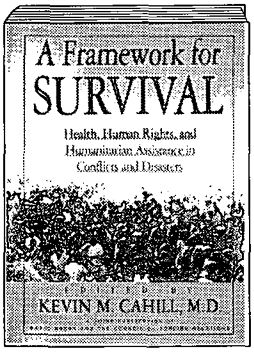
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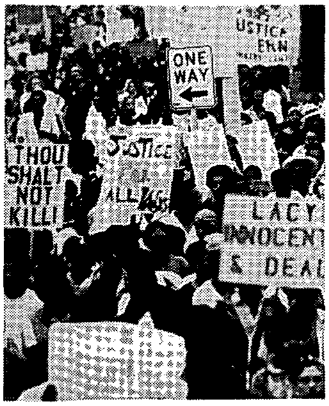

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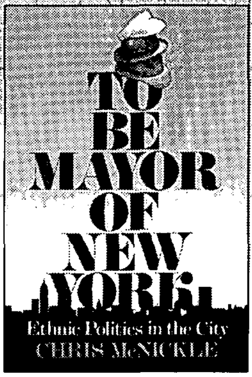

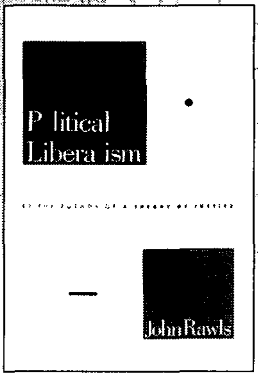

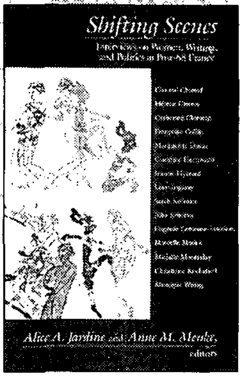

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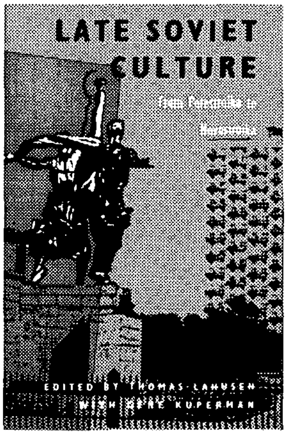
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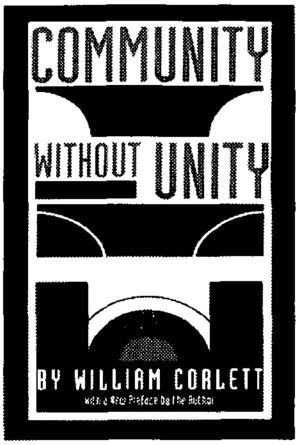
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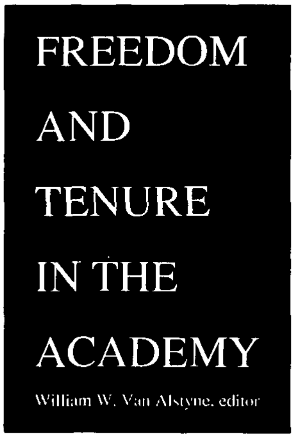

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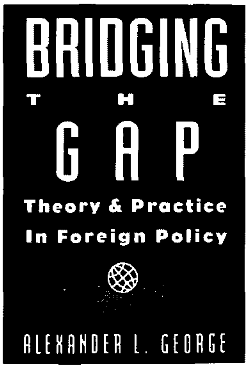

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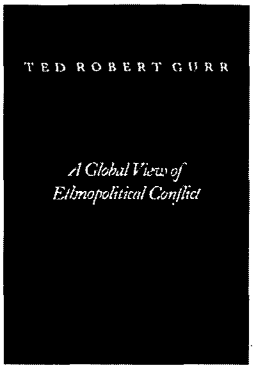

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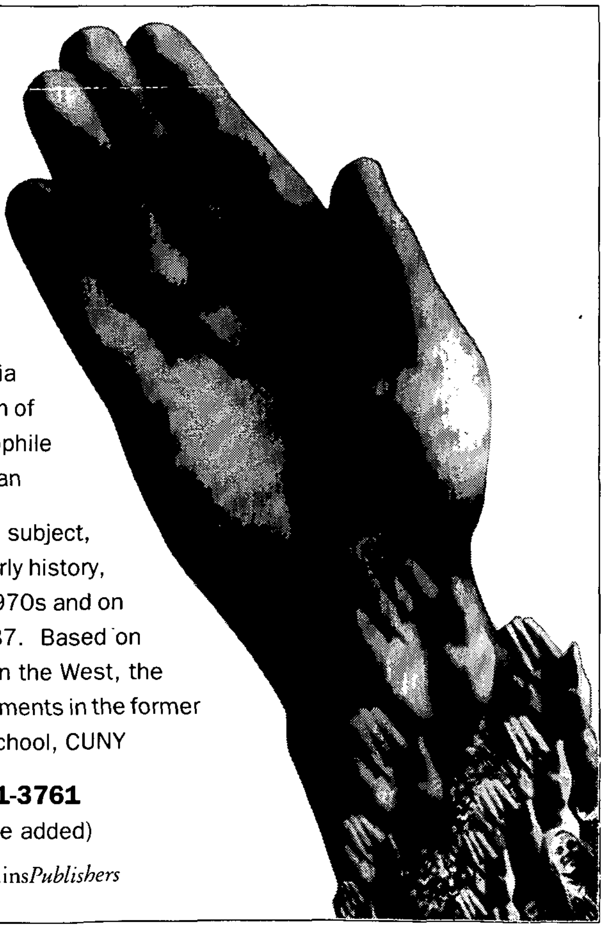

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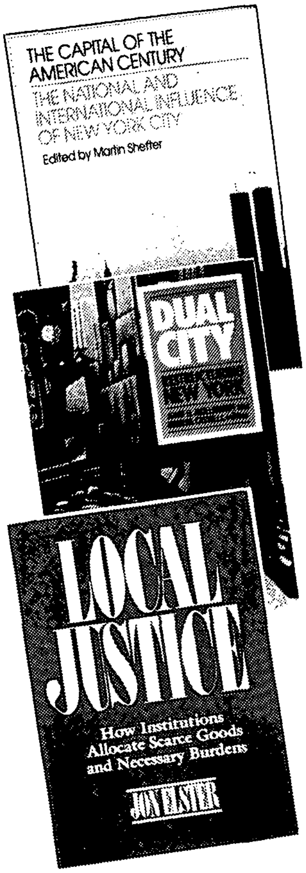

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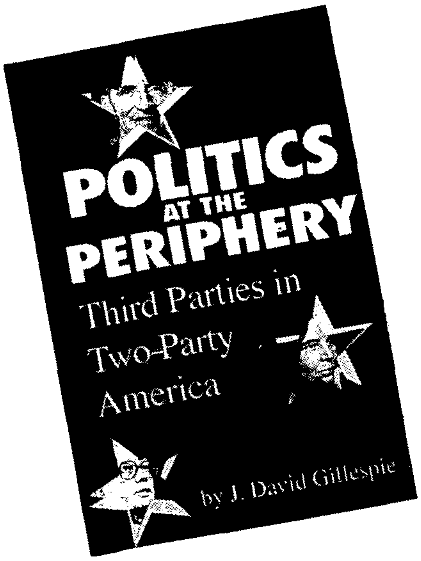

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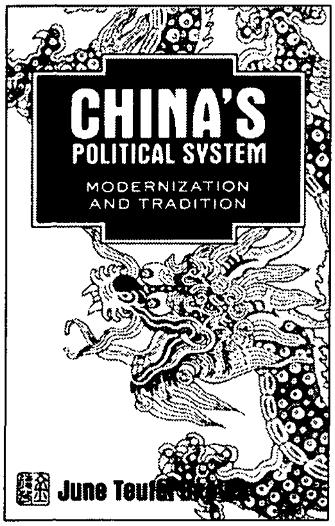

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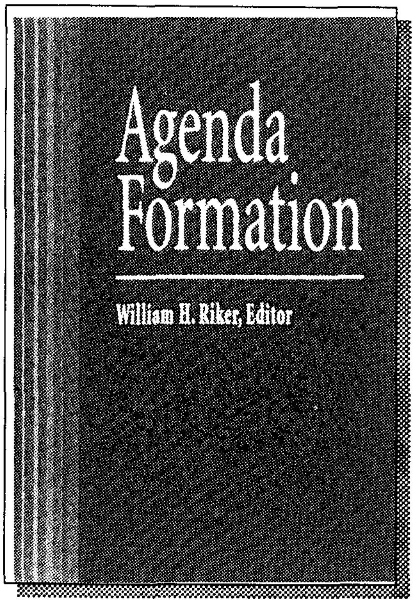

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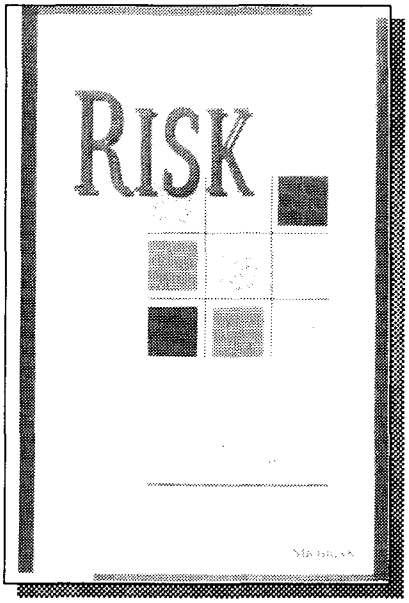

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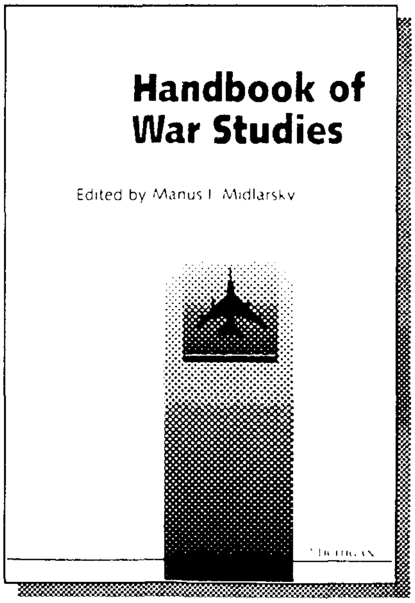

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